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A LOGICAL ANALYSIS OF 'PSYCHOLOGICAL ISOMORPHISM'*

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THE concept of isomorphism plays a prominent rôle in logic and mathematics and accordingly it has received a good deal of historical and systematic analysis. The concept also plays a prominent rôle in some empirical sciences, most notably in psychology, where, however, it has received neither an adequate historical nor systematic analysis. Historically, analysis is confined to one type of isomorphism or is interwoven with other material so it does not form a unit. Systematically, analysis often is preoccupied with doubtful *a priori* judgments, pro and con.[1]

After making preliminary statements about mathematical isomorphism, for the sake of subsequent comparison and contrast, I will provide what I take to be the elements of a historical and logical analysis of the concept of psychological isomorphism. The Gestalt variety of isomorphism will figure prominently in this analysis.

I *Different Types of Isomorphism*

The mathematical idea of isomorphism has two components. First, there must be a one-to-one correspondence between the elements of the two interpretations—or, as I shall also say, domains—which satisfy a set of axioms. Two sets of objects are in a one-to-one correspondence if they are paired off with each other so that to each element of the first set corresponds one and only one element of the second, and conversely. So one can say that the set of all mothers is in one-to-one correspondence with the set of all eldest children, whereas the

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* Received 18. ii. 57

[1] Cf. E. G. Boring, 'Psychophysiological Systems and Isomorphic Relations', *Psychological Review*, 1936, **43**, 565 sqq.; R.W. Erickson, 'Isomorphism as a Necessary Concept', *Journal of General Psychology*, 1942, **26**, 353 sqq.

correspondence between the set of all mothers and the set of all children is one-many. The second feature of isomorphism is that the different interpretations of an axiomatic set exhibit the structure specified by the axioms, although the interpretations may be very different in all other respects. Cohen and Nagel, for example, in their discussion of isomorphism give one arithmetical and one geometrical interpretation of a few axioms of projective geometry. Then they proceed to give the following general definition of isomorphism :

> Given two classes S, with elements a, b, c, . . . and S', with elements a', b', c', . . . ; suppose the elements of S can be placed in one-one correspondence with those of S', so that, say, a corresponds to a', b to b', and so on. Then, if for every relation R [that is, in the axiomatic system] between elements of S (so that, for example, $a\ R\ b$) there is a relation R' between the corresponding elements of S' ($a'\ R'\ b'$), the two classes are *isomorphic.*[1]

What the two authors call the two (empirical) relations R and R' are, of course, in my way of speaking, the two ' interpretations ' of one relational predicate of the axiomatic system in two different domains. But the question now arises, what has become of this notion of isomorphism in psychology ?

There are several domains believed or claimed to be in some sense isomorphic, in which there has been great interest shown in the history of psychology. The first pair consists of the domain of stimulus events, on the one hand, and the domain of sensory (response) events, on the other. The study of the relation between these two classes of events was referred to by Fechner as ' outer psychophysics ' as distinguished from inner psychophysics which concerns the relation of sensations to neural excitations.[2] However, the relation between the events of the two domains of outer psychophysics, as the classical psychologists conceived it, is not isomorphic in the full sense ; at best it could be said that some classical psychologists proceeded on the assumption of a one-to-one relationship between stimulus and sensory events. Further, it will have to be shown that according to more recent results *this* frame of reference is over simplified and is not borne out by the facts.

In the area which Fechner called ' outer psychophysics ' variations in the physical dimensions were found to be correlated with variations in sensory dimensions. As Boring has pointed out, the procedure of Fechner, Hering, G. E. Mueller, and others was to vary only *one* stimulus dimension at a time and to observe concomitant changes in a *single* sensory dimension. ' Out of such analysis grew the false belief that every simple dimension of sensation is correlated to a simple dimension of the stimulus—brightness or loudness to energy, quality to wave length or frequency.'[1] If it were true that each sensory attribute were determined by, or dependent on, a single stimulus dimension the sensory attributes and stimulus dimensions would be characterised as being in a one-to-one relationship. There is in this, besides the idea of one-to-one correspondence, also, of course, the idea of a causal dependency in that the physical event determines the phenomenal event. Such a one-to-one relationship could be asserted if hue were in fact dependent solely on wavelength, brightness solely upon energy of light, and saturation upon mixture of wavelengths. However it has been established that some such correlations are not one-to-one. The basic fact is that some sensory attributes are dependent on more than one stimulus dimension, e.g. hue varies with wavelength and energy as well. It is possible, then, to produce the same sensory character by different combinations of physical stimuli. The situation is further complicated where not only is there not a one-to-one causal relationship between physical and phenomenal events but there is not even the *same number* of stimulus dimensions as there are sensory attributes determined by them. Boring cites, for example, the study of S. S. Stevens on the attributes of tone. Stevens found that for a bi-dimensional stimulus (a tone having the two dimensions of frequency and energy) there are *four* sensory attributes—pitch, loudness, density, and volume. The Gestalters emphasise such cases of lack of one-to-one correspondence between stimulus and sensory response and conclude that something must be ' added ' in the organism in order to account for the response. This is their point of departure for speculation about what must ' happen ' in the organism.

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[1] *An Introduction to Logic and Scientific Method,* New York, 1934, p. 139

[2] Cf. Boring, *Sensation and Perception in the History of Experimental Psychology,* New York, 1942, pp. 34 sqq., 50

Further, Boring points out that the number of physical dimensions which a stimulus may have places no restriction on the total number of attributes a sensation may have, although it does restrict the number of *independent* attributes. Take the case where the physical stimulus is bi-dimensional and yields four sensory attributes. Suppose the functions relating each of the sensory attributes to the two dimensions of the stimulus are found through experimentation. Knowing these functions we would need only to know values of two sensory attributes in order to determine the values of the dimensions of the physical stimulus and finally to determine the other sensory attributes. The minimum number of sensory attributes which must be known, in addition to the functions, in order to determine all the rest of the sensory attributes, are in this sense *independent* ; and the number of independent attributes depends on the number of dimensions of the physical stimulus. As Boring concludes concerning Stevens' experimental results to which we have referred :

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[1] Cf. Boring, *Sensation and Perception in the History of Experimental Psychology*, New York, 1942, p. 89

> The conclusion is that the [total] number of attributes of sensation is independent of the number of dimensions of the . . . stimulus. A bidimensional stimulus may yield a sensation with one or with *n* attributes. The sensation will also be bidimensional in the sense that any two of its attributes will then determine all the others . . .[1]

In such cases the only one-to-one relation that could possibly exist is between a complete set of sensory attributes, on the one hand, and the complete set of physical dimensions that determine them, on the other.

There are, however, other domains in psychology which are, or were, believed to be isomorphic in a *full* sense ; that is, domains which, in addition to one-to-one correspondence, allegedly shared certain ' structural ' characteristics. The first pair consists of the domain of receptor events, on the one hand, and the domain of afferent neural processes entering the brain, on the other ; the second pair consists of the domain of neural events and of phenomenal events. The first type of ' full ' isomorphism found its most important exemplification in certain nineteenth-century ' nativistic ' (i.e. non-learning) theories of space perception ;[2] the second type, usually called psychophysical isomorphism, found important exemplifications, in the nineteenth century, in the colour theory of E. Hering and, in the present century, in the numerous theories of Gestalt psychology and epistemology.[3]

(i) The domains which supposedly are isomorphic in nativistic theories of space perception are retinal or skin stimulation and the excitations in the afferent endings in the brain (leaving out the complication of depth perception). Such ' projection ' theories generally held that there is a one-to-one correspondence between the two sets of excited points and that the spatial relations of the retinal image are preserved in some topological fashion in the afferent fibres entering the brain. Also J. Bernstein proposed that the processes in the sensory surfaces of the skin were projected by nerve fibres upon the brain in a similar manner. These are cases of real isomorphism because they include in addition to a simple one-to-one correspondence between points on the receptors and points in the brain certain topological relations exhibited in both sets of events. Such projection theories were physiological models, speculatively formulated, designed to account for the perception of space. I am not here concerned with the question whether such theories are, in fact, right or wrong. They have been held by respectable scientists and there is no logical or methodological objection to them. On the other hand, it *is* important to point out that projection theories alone would not constitute a ' nativistic ' account of, say, space perception. In order for the spatially isomorphic domains in the first pair to account for space perception there must also be structural relations in common between the domain of brain events and the domain of phenomenal events. What I have in mind is this : the nativistic solution to the problem of how space is perceived depends upon projecting the spatial relations of receptor stimulation on to the brain, but in order that these patterns of stimulation of nerve endings entering the brain should be efficacious in giving rise to *perceived* space, an isomorphic relation between the domains of central nervous events and phenomenal events—psychophysical isomorphism—must also be assumed.

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[1] E. G. Boring, ' The Relation of the Attributes of Sensation to the Dimensions of the Stimulus ', *Philosophy of Science*, 1935, **2**, 243

[2] E.g. in the theories of Johannes Mueller and Julius Bernstein

[3] Gestalt theory is not only psychology *but also epistemology and metaphysics*

(ii) Psychophysical isomorphism, both historically and systematically, plays a most prominent rôle in the use of the isomorphism concept in psychology. As I have indicated, Hering, and others, and also the Gestalters, propounded their various theories within the framework of this sort of isomorphism. Their procedure, because of a paucity of pertinent physiological knowledge, was to infer from phenomenal experiences the nature of the correlative physiological process. That is, *first* one assumes an isomorphic relation to exist between these two sets of events ; *then* he uses features of the phenomenal world to postulate certain physiological events—isomorphic with phenomenal experiences—which will explain these features.

According to Köhler,[1] the isomorphism to which Hering and G. E. Mueller referred obtains between the ' logical abstracted ' orders of experiences on the one hand and physiological events on the other. This ' logical ' aspect of Hering's and Mueller's isomorphism consists in this : if a sound of a given pitch is presented a number of times at different intensities, and the various presentations are ordered on the basis of loudness, their logical order may be likened to a straight line. That is, the loudnesses proceed from softest to loudest without reversal. On the other hand, if colours are arranged on the basis of hue, the dimension is ' circular ' in ' form '—as in the colour cone. The principle of isomorphism states, then, for Hering and Mueller, that if the underlying physiological events were examined for the properties corresponding to these attributes, these properties would be found to exhibit the same order, linear in the one case, circular in the other. These ideas on isomorphism were explicitly formulated by Mueller in several ' psychophysical axioms ' which were, as Boring says,[1] accepted universally if implicitly. They state, among other things, (*a*) that underlying each state of consciousness there is a concomitant material (psychophysical) process ; (*b*) that for similar or different sensations there are similar or different psychophysical processes, respectively ; and (*c*) that for changes of sensation in a certain direction there are corresponding changes in psychophysical processes in the same direction.

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[1] Köhler, *Gestalt Psychology*, New York, 1947, p. 60 sqq.

The ' abstract ' or ' abstracted ' aspect of the Hering-Mueller isomorphism on which Köhler insists refers to the ordering of events within a particular sensory dimension on the basis of certain relations (louder, brighter, etc.). In such a situation the observer has a set to attend to a particular sensory attribute, say saturation, while the other attributes—hue and brightness—are disregarded. Since each stimulus gives rise to a sensation having potentially all three attributes, the experience of saturation under the conditions of the particular set is an ' abstract ' one and the ordering of the stimuli on the basis of the one attribute is in this sense an ' abstract ' ordering.

The principle of psychophysical isomorphism espoused by the Gestalters is, according to Köhler, ' more general and more concretely applicable ' than that of Hering and Mueller.[2] To explain his meaning he considers the example of three white dots on a black surface. ' This ', he says, ' is also an order ; but, instead of being of the merely logical kind, it is concrete and belongs to the very facts of experience.' [3] The claim then is that *there is also an isomorphism between phenomenally*

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[1] Boring, *Sensation and Perception in the History of Experimental Psychology*, pp. 88-89

[2] Op. cit. p. 60

[3] Ibid. p. 61

given relations, on the one hand, and certain relational features of their physiological correlates, on the other. If the point is made in this way one avoids at least the misleading suggestion that the ' logical ' order with which Hering and Mueller are concerned is not based on experience whereas the Gestalt isomorphism is. The simple point must be made that the sensory experience resulting from an analytical set is just as much a ' fact of experience ' as an experience without an analytical set. The ' logical ' or ' abstract ' aspect of Hering's and Mueller's isomorphism is the subject's concentration on the topological relations of non-relational characters—e.g. the series of directional gradations. Köhler need not quarrel with this. All he needs to do is *add* that experienced relations have their isomorphic physiological counterparts. Accordingly, Gestalt isomorphism claims in the case of Köhler's dot paradigm that the underlying physiological processes ' are distributed in a certain order . . . and . . . this distribution is just as symmetrical in functional terms as the group of dots is in visual terms.' [1] In other words, in the physiological processes there must be something corresponding to the perceived betweenness of the middle dot. Köhler formulates this characteristic emphasis in three ' principles '.[2]

I Experienced order in space is always structurally identical with a functional order in the distribution of underlying brain processes.

II Experienced order in time is always structurally identical with a functional order in the sequence of correlated brain processes.

III Units in experience go with functional units in the underlying physiological processes.

The terms ' experienced order ' in principles I and II and ' units in experience ' in principle III unfortunately do not capture all that is involved in Gestalt isomorphism. That is, if one interprets, as is natural, the terms ' experienced order ' and ' units in experience ' so that they apply to *experienced* relations or *experienced* units within a perceptual field, then it would not refer to and would not include cases of a relational but non-experienced character such as the field character and the character of a system altering its own medium. Since it seems that these principles are intended to cover these cases, the situation is worth exploring in some detail.

To give an example of Gestalt procedures : they take perceptual phenomena such as stroboscopic movement, perceptual grouping of *stationary* objects, and reversible figure-ground phenomena as requiring the concept of *phenomenal* field, by which is meant that events or objects located in one area of the visual field influence events in other areas 'in a way that depends directly on the properties of both in their relation to each other'.[1] Now this field character of visual experience, by means of the isomorphic principle, sets a requirement for the brain processes : the brain processes must have a physical nature which meets this requirement. Consequently Gestalt psychologists say that we must assume that physiological processes have the nature of a macroscopic physical system capable of dynamic interaction of the kind they hold is characteristic of fields—and electrolytic processes, they believe, best fill this bill. It is clear in the case of the perceptual grouping of stationary objects that we can say that a subject has one perceptual experience when stimulus entities bear certain relations to each other (similarity, proximity, and so forth), whereas the subject will see a different grouping when one or more of the relations among the stimulus elements are changed. But it is also clear that immediate experience does not include the observation of the elements in one part of a group *as depending* upon the other elements in the group in the way, say, that the spatial relations of 'before' and 'between' are a matter of immediate experience. In other words, the field character is not itself either a qualitative visual datum or a relational datum *within* the visual field like 'between' and 'to the right of'—characters exemplified by particulars within the visual field. Rather it is from *two* perceptual responses to two different stimulus situations that one *infers* that all parts of a system or group are interdependent in the sense that alterations of any one stimulus element or relation will affect how all the other stimulus elements are seen.

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[1] Boring, *Sensation and Perception in the History of Experimental Psychology*, p. 61

[2] Ibid. pp. 61–63

Let us recall Köhler's dot paradigm in which a matter of immediate observation, the experience of *a* being *between b* and *c*, sets the requirement which is to be met by the physiological processes. One dot is seen between the other two and this relation is part of immediate observation in the same way that the whiteness of the dots is a matter of immediate observation. Köhler says that there must therefore be something in the physiological events to correspond to this visual experience of betweenness. He writes, '. . . our principle refers to the relation between concrete experienced order and the underlying physiological processes.'[2] However, we can see in the case of the field character that it is not always (and in fact it is not usually) a concrete experienced order which sets the requirements for physiological processes. Consequently the dot paradigm is not universal in the sense that it does not cover all the kinds of relations which are supposedly shared by the two domains of phenomenal and physiological events. If its natural interpretation is given to the term 'experienced order'—that is, if it is interpreted to refer to experienced relations in the various perceptual fields—then it does not refer to a relational but non-experienced character such as the field character. In another place, Köhler himself indicates that the phenomena which set the field requirements are not matters of immediate experience. He writes that 'We seldom experience much of the actual genesis of visual percepts, but we can observe what things or figures appear under different conditions of stimulation . . .'[1] We could say, then, that the conditions which cause certain perceptual phenomena are located in the brain field but unlike the physiological correlates of these phenomena the physiological conditions themselves do not have a phenomenal counterpart.

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[1] Köhler, *Dynamics in Psychology*, New York, 1940, p. 55

[2] Köhler, *Gestalt Psychology*, p. 61

As a result of our analysis it would seem that one must interpret the term 'experienced order' to refer simply to the fact that Köhler uses phenomenal experience, that is, unanalysed experience, rather than analysed experience, as his point of departure for isomorphic speculation. Historically this is easily understood as the heritage he has received from the line that leads from Goethe and Purkinje through Johannes Mueller and Hering to the founder of Gestalt.

2 '*A priori*' *Justifications of Psychophysical Isomorphism*

The interesting question remains, of course, why anyone should accept the hypothesis of psychophysical isomorphism, whether it be the Hering-Mueller type or the Gestalt variation. One might, of course, argue that the assumption is justified because it is scientifically fruitful. Suppose, Köhler says,[2] that an empirical law in psychology [designated $R(A, B)$] is discovered. The psychological terms will have their counterparts in brain processes (a, b) and the functional relationship R will be interpreted as r, in some form of physiological or physical interaction between a and b $[r(a, b)]$. The problem at this point is some specific determination of the nature of a, b, and r

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[1] Köhler, *The Place of Value in a World of Facts*, New York, 1938, p. 251

[2] Köhler, *Dynamics in Psychology*, pp. 49 sqq.

in lieu of adequate physiological detail. Specific assumptions about a, b, and r will give these processes a position within the ' system of concepts with which the natural sciences deal '. These assumptions will not only have the characteristics of A, B, and R which the theorist had in mind when he selected the assumptions about a, b, and r but also will yield ' implications ' as a result of their position in a larger structure of physical knowledge. *With deductive elaboration of these assumptions we find that we get further assumptions about the physiological processes, say* a', b', *and* r'. With isomorphism functioning as a return bridge, there should then be in the phenomenal realm R' (A', B'). If such a law already has been found independently in psychology, then (i) its theoretical connection with $R(A, B)$ becomes apparent and (ii) it acts as a ' partial verification of our physiological theory '. If no such law exists as yet in psychology, then an experimental situation must be contrived to test its existence.

I do not wish to guess how good or how poor the isomorphism hypothesis is from this viewpoint of scientific fruitfulness ; but I do wish to urge, since the concepts of this hypothesis are not inherently unclear, that it will be uselessness, and not *a priori* argument about the philosophical untenability of isomorphism,[1] that will remove this concept from serious consideration, if it is to be removed at all.

Conversely, the isomorphic assumption cannot be established by *a priori* or logical arguments either. This conclusion follows from the fact that there are significant empirical alternatives, in specific explanatory contexts, which do not involve the isomorphic assumption. To see this point, let us compare, e.g. the two classical colour theories of Hering and Young-Helmholtz. One of the points of departure for Hering's colour theory is the existence of four phenomenally fundamental or irreducible colours, the so-called primaries—blue, yellow, red, green—in addition to achromatic black and white. This suggested to him that there are three visual substances in the retina—yellow-blue, red-green, and black-white—which can undergo chemical changes in two antagonistic directions from an equilibrium point. All hues other than the six phenomenally fundamental ones were assumed to result from various combinations of these processes, that is, the excitation of more than one process. One principle of the psychophysical isomorphism on which Hering is operating is thus that to the four phenomenal ' primaries ' correspond four physiological ' primaries '. As is well known, Hering's hypothesis can be fitted to a large number of data including of course the arrangement of hues in a topological circle. *However it is also possible to account for (approximately) the same range of facts by the speculative model of Helmholtz which does not involve ' isomorphism ' with respect to ' primaries '.* For Helmholtz, unlike Hering, took as his main point of departure the physical facts of colour mixture discovered by Newton. And since three properly chosen colours mixed in various proportions match all other colours, Helmholtz assumed that not more than three ' primary ' processes, in the physiological sense of ' primary ', are operative in the visual apparatus. It is only fair to point out, however, that Helmholtz's model leads to a question which finds an immediate answer in Hering's, while this is not so in Helmholtz's model. This question is of course : which are the features that characterise the processes corresponding to the four phenomenal primaries ? I do not know what answer, if any, the Helmholtzians have given to this question, nor is this important. For the logical multiplicity of possible answers (certain maximal-minimal properties in the distribution of the three Helmholtz primaries) is very great. But then where in the case of such an answer is the isomorphism ?

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[1] Cf. p. 177, n. 1

Nevertheless, *a priori* arguments *have* been offered which allegedly establish the isomorphic assumption independently of the question of its scientific usefulness. For example, one might argue in this fashion.[1] ' Objective experience ', let us say, refers to a character *within the phenomenal field,* namely, to any conscious content—e.g. any ' thing ' percept—which does not immediately or ultimately refer to the ' self ' percept. Now the physicist has inferred from this *objective experience* the nature of the physical events which caused it. If there were no basic similarity, no isomorphism, between objective experience and physical events, then, of course, one could not ' infer ' or ' draw a picture of ' the nature of physical events. However, the phenomenal world is more ' closely related ', both causally and existentially, to the physiological realm than the physical, so *a fortiori* these two realms must be isomorphic and one is able to infer from the nature of the phenomenal the nature of the physiological.

This *a priori* argument for isomorphic speculation is open to criticism because, in fact, no isomorphic relation obtains between the phenomenal and physical realms, or, more precisely, between the givennesses of perception on the one hand and the theoretical concepts of physics on the other. For example, where is the isomorphism in any precise sense of the term between the movements of molecules and the immediately experienced character ' warm ' which one naïvely ascribes to physical objects ? All one can say is : first, in theoretical physics the concept ' temperature ' is co-ordinated to some function of the average velocity of the particles. Second, after the co-ordination of all thermodynamic concepts to various features of the particle model is completed one expects to derive the thermodynamic laws as special instances of the laws of mechanics by applying the latter to the model. Third, the only necessary requirement is therefore that the functional relations among the concepts of the model be mathematically the same as the ones between the corresponding concepts of what physicists call phenomenological thermodynamics. Fourth, it should not be forgotten that these latter concepts are already an elaboration of the notions applied to the pre-scientific behavioural environment and that, again, it is not clear why an isomorphism in any precise sense need obtain between this elaboration and its basis in untutored perception. As to the two domains of physical objects and phenomenal experience, then, it is not only not necessary but in fact false that the former mirrors the latter in the way Gestalt psychologists call isomorphic.

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[1] Cf. Köhler, *Gestalt Psychology*, p. 57 ; K. Koffka, *Principles of Gestalt Psychology*, New York, 1935, pp. 61 sqq.

To elaborate further this important point, consider the phenomenal experience of colour, on the one hand, and the physical events which give rise to it, on the other. On the basis of phenomenal experience the hues ' form ' a topological circle, represented in the colour cone, in which there are gradual transitions from one hue to a neighbouring hue and a doubling back of hues towards the one with which one starts. But what about the physical events which give rise to these experiences ? Of course, no such ' topological circularity ' is to be found in the physical basis of light and colour.

These arguments, it should be noticed, eliminate the basis of the whole *a priori* argument ; and so it is unnecessary to explore the hazy notion of the phenomenal field being ' closer ' to the physiological than to the physical field.

3 *The Nature of Gestalt Epistemology*

Gestalt psychologists invariably formulate and defend their isomorphic assumption within the framework of an epistemological dualism, namely, representative realism.[1] I think the isomorphic assumption can be stated within the framework of any epistemological system and that it would be wise to state it in commonsense terms since this epistemological framework, unexamined to be sure, is the one in which scientists ordinarily operate. However, since the Gestalters believe that representative realism is the only correct way to formulate the isomorphism hypothesis, it is necessary finally, to examine the grounds of their philosophical predilection.

Koffka says[2] that the difference between the phenomenal and physical environments ' coincides with ' or ' is equivalent to ' the philosopher's distinction between appearance and reality. If we carefully consider this claim, and clear away the scientific talk about transposition phenomena, etc. the substance of Koffka's argument for phenomenal—or behavioural—environment or ' appearance ' emerges as the traditional philosophical argument from illusion, the function of which is to establish the existence of ' sense data ' or ' percepts ' as distinct from physical objects. According to the philosophical dialectic, in cases of illusion *something* is being experienced and inasmuch as it is not the object it must be a percept or, in other formulations, sense data. In terms of a classic example, I see a straight stick partly submerged in water as bent. My experience of ' bent stick ' may not have a physical counterpart but nevertheless I am perceiving something —sense data or percepts. In Koffka's terminology, my ' behavioural ' environment is ' bent stick ' while the physical stick is not bent. The next step in the dialectic is to extend the claim that we apprehend percepts, and not objects, to all experiences. Koffka likewise universalises his behavioural environment : ' But every *datum* is a behavioural datum ; physical reality is not a datum but a constructum.'[3] And in cases of illusion he tells us that the organism is not acting in a behavioural environment only at the moment of illusion but that it has been acting in the behavioural environment all along.[4]

Koffka asserts that the particulars of awareness are always percepts and never physical objects, but how does he establish the ' always ' and ' never ' in his claim? In characterising certain experience as illusory he is assuming the validity of some other experience by which the illusion is discovered. In order to universalise his behavioural environment, then, he would have to supply, as he does not, in this

[1] Cf. Köhler, *Gestalt Psychology*, p. 57; K. Koffka, *Principles of Gestalt Psychology*, New York, 1935, pp. 61 sqq.

[2] Op. cit. pp. 33, 35

[3] Ibid. p. 35

[4] Ibid. p. 29

context, some second reason why the apprehensions in these cases of valid perception are still percepts.

Koffka and Köhler in other contexts, however, do try to justify the universalisation of their sense-data or percept claims.

The dualist, Köhler says,[1] is able to tell 'an impressive story' in which a representative element is present not as a philosophical theory but as a matter of physiological fact : stimulus energy impinges on nerve endings ; nerve energy is transmitted into the central nervous system ; phenomenal experience results. In one place, however, Köhler *appears* to realise that this does not establish representative realism. He admits that one might easily object that the use of words like 'objects' already presupposes the existence of a world which is independent of and represented by experience and that consequently his procedure is circular. This criticism is too hasty, however, Köhler continues, because the phenomenalist for example does not contend that such things as 'brains', 'nerves', and 'sense organs' are 'unreal' but rather insists that what is ordinarily known as a physical object must be *interpreted* as a phenomenal entity. He concludes, 'They cannot, for such reasons, refuse to listen to the Dualist's report. As a report, then, . . . [physiology] is common ground for both the Dualist and the Phenomenalist.' [2]

If we are permitted to put Köhler's argument in our own terminology then we could say he holds that 'the impressive story' is something every philosophical position has to take into account but that this story does not establish epistemological dualism. In spite of occasional passages that seem to acknowledge the familiar philosophical dialectic, however, Köhler does not seem to appreciate what seems to me an obvious truth, namely, that no amount of empirical data, as collected and interpreted by science, can establish an epistemological position directly. For example, he writes,

> Epistemological Dualism holds that percepts cannot be identified with physical objects, because percepts emerge only after many events have happened between the objects and the organism, in peripheral parts of the organism and eventually in the brain.[3]

However, that 'objective experience', or the world of 'thing' percepts, depends upon such things as physical energy impinging on receptors and transmission of neural impulses, is a matter of scientific fact, and in this sense having ' percepts ' is a matter of physiology and psychology. It is clear that a percept in this scientific sense is not what it is the percept of and that when you and I look at the same galvanometer we yet do not have the same percept. This argument, however, is irrelevant to an epistemological discussion. When a person points out that objective experience depends upon certain complicated processes in the organism he is asserting laws which belong to the body of scientific knowledge. In such assertions obviously one has already accepted what constitutes a valid basis of knowledge and consequently such assertions have no epistemological significance. Epistemologically the question is what constitutes the valid basis of knowledge irrespective of the causal genesis of that basis.

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[1] Köhler, *The Place of Value in a World of Facts*, p. 108. Cf. Köhler, *Gestalt Psychology*, pp. 22 sqq.

[2] Ibid. p. 109

[3] Ibid. pp. 140-41

One might try, of course, to establish as a philosopher and by philosophical argument a representative realism and, within its framework, what has sometimes been called ' a causal theory of perception '. However this sort of argument is much different from Köhler's galvanometer point. What we have called the causal theory of perception would hold that ' This is x ' is equivalent to ' This is caused by x ' where ' this ' refers to a sense datum or percept and ' x ' to a physical object. The essential point in such a contention is that the physical object that is singled out as the cause of what is immediately observed is not itself a matter of observation. In Köhler's discussion of the galvanometer illustration, however, the physical object and sense data are both aspects within experience and between which scientific correlations are discovered.

I conclude then that the Gestalt variety of representative realism, in which Gestalt psychologists present and defend their isomorphism hypothesis, not only does not bolster their hypothesis, but is itself philosophically dubious.

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THE PLACE OF HISTORICAL STATEMENTS IN BIOLOGY *

R. P. GOULD

1 *Introduction*

THE relationships between science and history have been the subject of discussion for a very long time. On the one hand are those who warmly endorse the close association of history and science ; and on the other are those who dispute the value of any such association.

For example, Bury asserted that [1] 'history is a science, no more and no less' ; and Collingwood suggested that [2] 'no one can understand natural science unless he understands history', and that 'no one can answer the question what nature is unless he knows what history is' ; and elsewhere he asserts, 'a scientific fact is a class of historical facts ; and no one can understand what a scientific fact is unless he understands enough about the theory of history to understand what a historical fact is'. But Oakeshott is just as emphatic that [3] 'wherever science and history have been associated, nothing but error and confusion has followed', and that in logic such a conjunction would be bound to lead to the production of valueless hybrids.

For biologists these viewpoints are interesting, because although their subject is an empirical science yet of all the natural sciences it is the one where historical statements are most thick on the ground. This would seem to imply that a part of the subject matter of biology is historical in character. Because of this it would seem desirable to analyse the place of historical statements in biology, for it would help make explicit and show the fruitfulness or otherwise, of what has hitherto been a somewhat confused area of biology ; further, such an analysis may help to shed some light on the general usefulness of relating history and science and so enable something definite to be asserted about such relationships.

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* Based on a paper read at the first annual conference of the Philosophy of Science Group, Manchester, 23rd September, 1956.

[1] J. B. Bury, Inaugural Lecture, Cambridge, 1903

[2] R. G. Collingwood, *The Idea of Nature*, Oxford, 1945, p. 177

[3] M. Oakeshott, *Experience and its Modes*, London, 1933, p. 108

Before beginning the analysis of historical statements in biology, the meaning of the terms history, science, and biology, as used in this paper, will be stated, and some consideration given to the general characteristics of the statements which make up history and science.

First the definitions :

(i) By history is meant that branch of knowledge which consists of statements that are descriptions of objects and events which existed or occurred in the past, and other statements which explain the occurrence of past events.

(ii) By science is meant that branch of knowledge which consists of descriptions of various aspects of phenomena, generalisations of these descriptions, and explanatory hypotheses which have as consequences the generalisations to be explained.

(iii) By biology is meant that science which consists of statements from zoology, botany, anthropology, palaeontology (including that part which deals with the prehistory of man), and certain relevant parts of neighbouring sciences. Applied biology, e.g. medicine and agriculture, and psychology, are not included in the definition.

As so defined it would appear that parts of biology—an empirical science—consist of historical statements, for the whole of palaeontology, parts of anthropology, and the evolutionary studies that deal with phylogeny consist of statements of the kind which have been defined as historical. For an empirical science to consist in part of statements, and in particular explanatory statements about the past, is an unusual state of affairs, because the statements of most of the other natural sciences make little or no reference to any particular past temporal specifications.

Biologists view this situation in a variety of ways. Some consider that historical statements in biology are an integral part of biology and are not significantly different from other biological statements. Whereas, other biologists recognise that historical statements in their subject play a different rôle from the scientific type of biological statement, yet still think of them as an integral part of biology. The important point, therefore, is this. If such a mixing of scientific and historical statements does indeed occur in biology, what methodological problems arise out of the mixing? And is such a mixing useful or harmful to the development of biology?

Before answering these questions the characteristics of the statements found in history and science will be briefly discussed.

2 *Characteristics of statements found in natural science*

The classification used here is one suggested by Woodger[1] who recognises four kinds of statement.

(i) *Observation Records*

These are singular statements and are records of particular observations of particular things in particular places at particular times.

(ii) *Universal Generalisations of Observation Records*

These statements are hypothetical in that they are generalisations covering a vast range of instances past, present, and future. Except in the comparatively trivial case where the possible instances are identical with the actual observed instances, most of the instances are inaccessible to us.

(iii) *Explanatory Hypotheses*

These are universal general statements, which at the lowest level[2] have as logical consequences the generalisations of observation records which are to be explained, and at the higher levels have as their consequences other lower level explanatory hypotheses.

(iv) *Consequences of Explanatory Hypotheses* (*other than those under* (ii))

These are new universal general statements derived deductively from explanatory hypotheses. They are sometimes called predictions as they lead eventually to the making of new observations (i.e. possible instances of the new universal general statement) which are said either to support the explanation further, or to falsify it.

3 *Characteristics of statements found in history*

These statements are more difficult to characterise than scientific statements because they are often only implicit in any piece of historical writing. Hence examples of the different kinds of historical statement are not often to hand but have to be worked out from a particular piece of historical writing.

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[1] J. H. Woodger, *Physics, Psychology and Medicine*, London, 1956, p. 32

[2] The 'level' of an explanatory hypothesis in an axiomatised scientific theory is largely determined by the consequences derivable from it and also from whether it is itself a consequence of some other hypothesis. The 'higher' the level of the hypothesis the more and wider the spread of its consequences—and vice versa.

The following tentative classification of the statements found in history is proposed.

(i) *Descriptive Statements*

These are singular statements which describe objects or oral noises which later may be interpreted as being traces or records of objects or events which existed or occurred in the past. These statements are identical in form with the observation records of natural science, although they may emphasise features not usually described in the typical observation records of natural science.

(ii) *Interpretive Statements*

Interpretive Statements are crucial in any discussion of particular historical statements. They are usually implicit in any piece of historical writing, yet when taken with Descriptive Statements enable Statements about the Past to be made.

The following is an example of a simplified Descriptive Statement and various Interpretive Statements associated with it showing how a Statement about the Past may be constructed :

'This piece of rock with certain petrological features was found in a certain geological context, has a definite form and is marked in a particular way.'

(iii) *Interpretive statements asserted about the Descriptive Statement*

(*a*) From certain universal general statements about the petrological, geological, and chronological characteristics of rocks similar to our specimen, we assert :

'The specimen is a sedimentary rock of a . . . age.'

(*b*) From explanatory hypotheses of the way fossils are formed from animals that died in newly forming sedimentary rocks, we assert :

'The specimen of rock is the fossilised remains of a once living animal.'

(*c*) From universal general statements about the anatomy of present-day living animals, we assert about the particular markings on the fossil : 'They indicate that the once living animal belonged to a . . . phylum of animals.'

If these three Interpretive Statements are now put together we can assert that our specimen of rock with its various interpreted characteristics provides sufficient grounds for making the following Statement about the Past :

' An animal of . . . kind, and possessed of the following structural features, lived at . . . a period of time.'

For this example it can be seen that Statements about the Past largely depend upon the number of Interpretive Statements that can be asserted about the subject matter of Descriptive Statements. The more Interpretive Statements available the fuller the description that can be made about the events or objects alleged to have taken place or to have existed in the past.

Two further important points must be made. If Statements about the Past are formulated on the basis of implicitly made Interpretive Statements about the subject matter of Descriptive Statements then the assumptions on which the Interpretive Statements are based must be clearly stated. This is important because as the assumptions underlying Interpretive Statements are either universal generalisations of observation records or explanatory hypotheses the degree of confidence which can be attached to them will determine the reliability of the Interpretive Statements. The second equally important reason for stating clearly the assumptions on which Interpretive Statements are based will be seen from the following examination of Cuvier's Law.

The so-called historical principle known as the Law of Morphological Correlation is used by palaeontologists in the interpretation of their data. This principle, first stated by Cuvier in 1812, is in fact a biological universal generalisation which can be further confirmed or falsified by new observations on living things.

It asserts that :

> ' Every organism forms a whole, a single closed system, all the parts of which are in mutual correspondence. None of these parts can change without the others changing also, and consequently each of them taken separately indicates and gives all the others.'[1]

For palaeontologists the last clause is the important one ; it forms[2] ' one of the most fundamental principles of palaeontology '. It means that whole animals can be identified from parts of them. Given a jaw fragment of an unknown animal you can, with care and good fortune, determine that it belongs, for example, to a new genus, and place it in a previously defined family. Unfortunately the principle has come to mean that, given one kind of fossil part, e.g. an ungulate hoof, when this is found associated with other fossil parts, these will be of the same kind, i.e. ungulate teeth or shoulder blades. And from this belief has sprung the idea that given the small fossil fragment the rest of the extinct animal could be reconstructed ; the reconstructions, of course, becoming even more convincing if further fossil parts are found which fit the reconstruction.

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[1] P. M. Cuvier, *Recherches sur les ossements fossiles de quadrupèdes*, 1812

[2] G. G. Simpson, *Symposia on Quantitative Biology*, 1950, **15**, 57

Cuvier's Law is thus used by palaeontologists as a basis for making Interpretive Statements about their fossil material and the reconstruction of the animals from which they originally came. However, the difficulty arises when the reconstructions are used as confirmation of the Law of Morphological Correlation. The law is thus used in the first instance in an interpretive way on certain fossil remains and then it is asserted that the results of this treatment confirm the law. Unfortunately (or perhaps fortunately) a number of counter instances are known which illustrate the law's limitations when applied to fossil material. For example fossil ungulate hooves have been found associated with fossil edentate heads, a finding which obviously falsifies the law in its application to all fossil material. It is perhaps worth noting that if the law is strictly adhered to one of its consequences would be that evolution has not occurred among organic things.

Thus the second reason for stating clearly the assumptions on which Interpretive Statements are based is that if a Statement about the Past (built up from Interpretive Statements) is to be used as confirmation or falsification of a scientific or historical explanatory hypothesis, and if the assumptions behind the Interpretive Statement are the same as those used in asserting the explanatory hypothesis, then the essential part of the explanatory hypothesis, that has to be confirmed or falsified, may be written into the constructed Statement about the Past which is used to test the hypothesis. It is essential, therefore, that the basis for the acceptance of Statements about the Past be independent of the assumptions used in asserting any explanatory hypothesis with which Statements about the Past may be associated.

Thus the chief characteristics of Interpretive Statements are :

(*a*) They are hypothetical statements.

(*b*) They are usually derived from other universal general statements.

(*c*) They assert that the subject matter of Descriptive Statements is an instance of universal general statements which are either generalisations of observation records or explanatory hypotheses, of the kind found in the natural sciences.

(*d*) They can be associated with Descriptive Statements, which enables new hypothetical statements to be constructed. These are called Statements about the Past.

(*e*) Usually one of the Interpretive Statements associated with a Descriptive Statement makes some assertion as to the subject matter's date of existence or occurrence.

(iv) *Statements about the Past*

These appear at first to be mainly singular statements, at least in ' history proper '.[1] But biological historical statements are not necessarily singular, as will be argued further on. To take one simple example. From the singular statement that ' This Tyrannosaurus was a carnivore ', the universal general statement that ' All Tyrannosaurs were carnivores' can be asserted ; and it would be falsified if further Tyrannosaur fossil skulls with herbivorous teeth were unearthed.

Statements about the Past, in a way previously outlined, are constructed from Descriptive and Interpretive Statements, and are therefore hypothetical.

4 *Statements called Historical Explanations*

Historians argue whether their main task is simply to discover what they call ' past events ' and to write a narrative or plain account of them, or whether it is to write an account which not only records but also connects past events into a significant story which explains how and why the events happened. Most history written today seems to be of the latter kind and it will therefore be worth while discussing historical explanation in some detail and comparing it with the kinds of explanation that are found in the natural sciences. This perhaps will help towards understanding how the words history and science are used (thus indicating whether history can be a science or not) as well as towards discovering how historical explanations are used in biology.

It is often said that history is first of all concerned with particular events, with what happened on this or that particular occasion. If this is so then historical explanations can deal either with these particular events as instances in a narrow historical context or as instances of some universal generalisation, providing this can be shown. From this it follows that historical explanations can consist theoretically of statements of two kinds :

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[1] By ' history proper ' is often meant the history of man's political activities

(i) *Universal General Statements*

These are similar to, if not identical with, the form of explanatory hypotheses in the natural sciences.

In science a low-level explanatory hypothesis has two sets of consequences. Firstly, there are the universal generalisations of observation records, to explain which the hypothesis was first formulated. And, secondly, there are new consequences derived from the explanatory hypothesis which again are universal general statements, but not of actual observations that have been made. These new consequences (sometimes called predictions) may lead to possible observable instances that would either confirm or falsify the prediction and so in turn either add support to or throw doubt on the original explanatory hypothesis.

(*a*) *Statements about the Past as Observation Records*

Now, if Statements about the Past are accepted as accurately describing past events (i.e. if they are thought of as being similar to observation records) and if they can be shown to be instances of a universal generalisation for which there is an explanatory hypothesis, then these Statements about the Past may be said to be explained by the hypothesis, for they are instances of a universal generalisation which is a consequence of the explanation.

On the other hand, it is possible to think of this process of explanation in what might be called the reverse direction. Suppose, for example, a new universal generalisation were derived from an explanatory hypothesis and if, instead of making a new observation which was an instance of the generalisation, a Statement about the Past well founded on Descriptive and Interpretive statements were known or postulated, then it may be said that this Statement about the Past is an instance of a retrodiction [1] of the original explanatory hypothesis. This Statement about the Past may then be said to describe an event which confirms or falsifies the explanatory hypothesis in a way similar to that in which an instance of a prediction is said to confirm or falsify an explanatory hypothesis.

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[1] ' Retrodiction ' is synonymous with ' post-diction ' as used by Woodger (J. H. Woodger, *Biological Principles*, 1929)

Retrodictions are obviously not as convincing as predictions as confirmation or falsification of an explanatory hypothesis because a Statement about the Past (which is highly hypothetical) is being used as though it were an observation record.

(*b*) *The Uniqueness of Statements about the Past: and their Generalisation*

Woodger in his *Biological Principles* takes up a different attitude to this kind of historical explanation. He says :

> History deals with the particular features of unique occurrences as such, and consequently genuine historical data alone are of primary value. Inductive generalizations (i.e. Universal generalizations of observation records and explanatory hypotheses) deal only with pervasive general processes which are exemplified in many events and consequently are of no direct use to the historian.[1]

Further, assuming that the laws of nature were the same then as now, they do ' not give us genuine historical data about the precise details of the *actual* historical occurrence ' . . . for without . . . ' historical data we can make no historical use of the inductive laws '.[2]

It will be argued here that although Woodger's arguments do often apply (particularly to the history of man) they do not necessarily apply to all historical events of a biological, sociological, and economic kind.

Firstly, Woodger's arguments only apply if it is accepted that all Statements about the Past describe unique events and so are incapable of being usefully generalised. It is suggested here that this is not necessarily the case. Of course it must be admitted that two historical events will always differ (i.e. be unique) in at least one important way even if remarkably similar in all other respects ; and this is a spatio-temporal difference—one event will occur at one place and time and the other at a different place and time. But in science spatio-temporal differences in events are to a large extent ignored.

To illustrate this argument let us examine two Statements about the Past to see whether they are ' so unique ' that they cannot be usefully generalised.

Firstly, if the following Statement about the Past, i.e. that ' Charles I of England was beheaded in 1649 ', is accepted, few would deny that it was a statement describing a unique and singular event which could hardly be usefully generalised. The statement, of course, could be generalised in such an assertion as, ' All Charles I's of England were beheaded in 1649 ' but it would be a trivial universal generalisation since the possible instances are identical with the actual known instance ; most scientific universal generalisations are, however, more pervasive than this.

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[1] Woodger, op. cit. p. 399

[2] Ibid. p. 399

As a second contrasting Statement about the Past the following example will be used :

' The blood poured from the severed neck of a man beheaded in 1649 not in a continuous stream but in twin spurts.'

Now the important difference between these two examples of Statements about the Past is that in the case of the description of the way blood spurted from a severed man's neck it is a description of an event which except for certain minor features could be (and has been) repeated at any time ; but that in the case of the beheading of Charles I the event, except for certain minor features, could never be repeated again. Or to put it loosely, the event of Charles I being beheaded was ' more unique ' than that of the way blood is lost from a severed neck.

These differences in *uniqueness,* if it can be put this way, obviously depend upon how fully, either implicitly or explicitly, the events in question are described. This notion of the fullness of the description of an event determining its uniqueness, or, to put it another way, the pervasiveness of the generalisation that can be made from the statement describing the event, has important consequences for the argument that some Statements about the Past may be explained by certain scientific explanatory hypotheses.

Explanations of Statements about the Past will depend in the first instance upon the fullness of the description (or degree of abstraction of the past events) contained in them. The historical event of Charles I being beheaded needs much detailed description for the event to be sufficiently characterised. Hence any generalisation of this statement is trivial because the range of the generalisation is limited, thus making very difficult the construction of suitable hypotheses to explain the generalisation.

In the case of the description of the way blood flowed from a severed neck, however, relatively little detail is necessary for the event to be sufficiently characterised. This means that singular descriptive statements about this past event can be usefully generalised and they will be widely pervasive generalisations. The possibility of constructing suitable hypotheses to explain such generalisations is thus made easier.

(c) *Uniqueness of Statements about the Past and Causal Explanations of Evolution*

An important particular biological consequence of this argument applies to the problem of whether causal explanations of the past evolution of once living things can be reasonably asserted.

Woodger on this problem argues that 'we only discover causal regularities by putting things in certain relations and observing what happens, and then repeating a similar series of changes with similar things but with certain constituents omitted'; [1] and if this is coupled with the notion 'that evolution represents a unique series of unrepeated changes which are past', it is difficult to see 'how we can find the causal regularities of evolution'. (Woodger's remarks presumably only apply to the evolution of once living animals, and there would seem to be little reason why in theory it should not be possible to discover causal explanations of the future evolution of present day living things.)

Popper expresses similar views where he says:

> The evolution of life on earth, or of human society, is a unique historical process. Such processes . . . can only be described by a singular historical hypothesis. Universal laws make assertions concerning some unvarying order . . . In other words, we cannot hope to test a universal hypothesis nor to find a natural law acceptable to science if we are confined to the observation of one unique process.[2]

The arguments of both Woodger and Popper depend firstly upon how causal regularities are discovered and tested and secondly on the assumption that 'evolution represents a unique series' of unrepeated events.

But, it has been argued, the 'uniqueness' of a so-called unique event or change depends to a large extent upon the fullness of the description of the event; and it would therefore seem at least theoretically possible to take as Statements about the Past descriptions of historical events or objects that can be usefully generalised. And if such generalisations are consequences of existing or newly invented explanatory hypotheses then these Statements about the Past, as instances of the generalisations, will thus be explained by the hypotheses or causal laws.

The question as to whether there can be casual explanations of past evolution depends upon three important things:

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[1] Woodger, op. cit. p. 399

[2] K. R. Popper, 'The Poverty of Historicism', *Economica*, 1949, 12, 70

(*a*) What it is that has been said to have evolved, e.g. a herbivorous dentition or a particular mammal-like reptile.

(*b*) What historical traces there are for the things that have been said to have evolved.

(*c*) Whether the statements about the evolved things can be shown to be instances of a universal generalisation which is a consequence of an existing or newly-formed hypothesis.

(ii) *Historical Explanations that are Singular Statements or disjunctions of Singular Statements*

(*a*) *Characteristics of this type of Explanation*

The second kind of historical explanation usually consists of a singular statement, or several singular statements. Historians employ this kind of explanation when examining the historical context of some particular past event. They look for antecedent factors that could account for the event, and usually one, or a few, of these factors is seized upon and it is asserted that it was a necessary prior condition for that particular event's occurrence. Often these antecedent events have been unnoticed or undervalued by other historians and it is the value of the explanation to point out their importance as a necessary, if not sufficient, reason, for the event in question to have occurred at all. This kind of explanation is obviously different from that previously outlined, for it asserts that for a particular event to have occurred such and such prior events or conditions were necessary. It does not assert that given the necessary condition the event in question could have been predicted. Gallie calls this kind of historical explanation[1] 'characteristically genetic explanations'. They are, he suggests, characterised by their pointing out some kind of continuity between one or a number of prior conditions and a subsequent result; they emphasise the one-way passage of time but do not constitute both a sufficient and necessary condition for the occurrence of some subsequent event. Finally, Gallie remarks,

> we look for the antecedent event, the explicans, which can be accounted a necessary condition of the explicandum, either on ordinary inductive grounds (i.e. observation of analogous cases) but more specifically on the grounds of a probable continuity—in respect either of persistent factors or of a direction of a change—between the explicans and the explicandum.[2]

[1] W. B. Gallie, *Mind*, 1955, **64**, 163

[2] Ibid. p. 166

(*b*) *Occurrence of this type of Explanation in biology*

In biology this kind of explanation is found quite commonly both in palaeontology and embryology. In palaeontology the form of a particular species of animal living at a certain time in the past as evidenced by its fossil remains, is often explained by reference to a series of preceding fossil forms of animals existing prior to the animal in question, and between which it is assumed there is a genetic continuity. No biologist, however, would assert that this was a complete causal explanation of the animal's form. All that this type of explanation does is to suggest that a number of animals of such and such forms were a necessary condition for the development of the form of the animal in question.

In a similar way the form and relationships of a particular structure in present-day living animals is explained. The explanation involves the description of a series of embryological events that occurred in the early development (or history) of the animal ; and it is asserted that the embryological events were a necessary but not a sufficient reason for the attainment of the animal's definitive form. Again no embryologist would be satisfied with this as a complete causal explanation, and indeed the dynamic behind most experimental embryology is the desire to find the causal explanations lacking in this branch of morphology.

5 *Kinds of Historical Statements in Biology*

If this account of historical explanation is reasonably well founded it will now be worth examining in its light some of the various kinds of historical statements and explanations that are to be found in biology.

(i) *Biological Historical data*

Before doing this, however, something must be said about the historical biological data which are available to the biologist. Data of this kind are restricted mainly to the fossil remains of the hard parts of once-living things, although certain soft parts in a number of cases have also been well preserved. Besides this there are a certain number of data available from which tentative statements can be made about the kind of environment and climate in which organisms once lived.

On the other hand historical traces from which statements can be made about the physiology, behaviour, biochemistry, psychology, and genetics of now extinct organisms are almost non-existent. In the nature of things this is not surprising.

(ii) *Historical statements concerning the form of organisms*

Statements about the form of extinct animals may be shown to be instances of morphological universal generalisations about living animals that are consequences of certain explanatory hypotheses. If this is the case then these statements about the form of now extinct animals may be said to be explained by these hypotheses. For example, certain measurements on say the fossil remains of the giraffe's ancestors may be explained in this way by the laws of allometric growth.

Conversely it is possible to retrodict from, say, the laws of allometric growth to discover whether or not the form of now extinct Dinosaurs was also governed by these laws. It may even be possible solely from the examination of fossil material to formulate new morphological explanatory hypotheses which could be further and more convincingly tested by the making of observations and experiments with present-day morphological phenomena.

In considering how the form of a particular species of animal has come about, that is its embryology and evolution, at least four kinds of explanation may be used.

As has been suggested, there are two kinds of explanation possible in embryology : the characteristic genetic explanation ; and causal hypotheses explaining various aspects of the ' genetic explanation '.

Then with regard to the evolution of an animal's form, a characteristically genetic explanation may be put forward which merely seriates the fossil remains of animals living prior to the species in question and assumes them all to be genetically connected. And lastly, explanations may also be formulated which account causally for the various changes through which this genetically related series of animals has undergone. These latter causal explanations are founded on information derived from processes going on now in and between living animals ; retrodictions are then made from these laws and applied to the available historical data. Unfortunately the results of this kind of treatment are often not scientifically or even historically sound because the historical data necessary for such explanations are either scanty or completely lacking and hence often have to be invented or assumed to make these causal explanations plausible.

A recent example of this kind of explanation is to be found in an account of the evolution of cartilage and bone. Pritchard, discussing this point, outlines the historical evidence relating to the question of whether bone evolved from cartilage or vice versa ; or whether they evolved simultaneously. He states that until recently it was thought that the earliest vertebrates possessed a cartilaginous skeleton, but that newly discovered remains provide evidence that even earlier vertebrates with a bony skeleton existed. Hence, it is likely that bone evolved before cartilage. This is a 'characteristically genetic explanation'.

However, Pritchard goes on to give possible causal explanations as to how bone and cartilage came to be evolved. He suggests, from data collected from living organisms, relating to the composition of bone and cartilage, fracture repair, and the normal development of these tissues[1] that 'perhaps the two evolved together, *because of an early mutation having given rise to a cell type able to manufacture either matrix according to the dictates of environmental factors*'. On the other hand, he continues, 'there may be more than one form of cartilage-making cell, only one of which is a variant of the bone-making cell, the others having evolved separately'.

This is obviously an extension backwards to now extinct organisms of certain current explanatory hypotheses about mutations, and the reactions of cells in different environments. Now, as has been suggested, it is possible to use causal hypotheses to explain Statements about the Past, but it is crucial in doing this for supporting historical evidence to be available.

In the example just given, two possible hypotheses are put forward as causal explanations of how bone and cartilage evolved. Unfortunately, there is no available historical evidence for the support of either of them. Indeed, it is very difficult to see how there could be any historical evidence for the possible cell types that may have developed into bone or cartilage some 90 million years ago, let alone any for some early mutation that occurred among these cells.

As a further example of retrodicting universal generalisations about the features of certain living animals to now extinct animals, as an account of the course of evolution of some group of animals, de Beer's explanation of the 'Evolution of the Metazoa'[2] will be examined. This explanation is based on the pioneer researches of Jovan Hadzi into the structure and development of the lower animals. Until recently the accepted view of how the Metazoa evolved was that the Protozoa evolved through the Coelenterata (simple 'two layered' animals) to the more complicated 'three-layered' Metazoa. The new hypothesis suggested is that multinuclear Protozoa evolved through the Turbellaria Acoela into the more complicated Metazoa, the Coelenterata not being on the main evolutionary line.

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[1] G. H. Bourne (Ed.), *Biochemistry and Physiology of Bone*, 1956, p. 4

[2] G. R. de Beer, *Evolution as a Process*, 1954, p. 24

The arguments in favour of either of these views consist almost entirely of a comparison of the characteristics of the present-day living representatives of these groups and the assertion that because one or more of the characteristics of one group are more 'primitive'[1] than those of the other group, therefore the ancestors of the first group lived before those of the second.

For example, radial symmetry is assumed to be more 'primitive' than bilateral symmetry; therefore the ancestors of radial symmetrical animals are said to have evolved before those of bilaterally symmetrical animals.

All the arguments are basically of this type, and as no historical evidence, i.e. fossil evidence, is available to back them it is possible to hold the opposite views without the least fear of being contradicted.

Explanations or speculations of the above kind seem to be of little value to the understanding of past events unless they stimulate the making of new hypotheses that could be tested with observations on present-day phenomena. To take our first example, if this speculation suggested that a certain type of undifferentiated cell might develop into cartilage or bone according to the environment in which it found itself, and if this hypothesis was supported by further experiments, then new and valuable information would have been obtained.

(iii) *Historical Statements concerning the Physiological and Biochemical Phenomena of Organisms*

It is suggested here that it is legitimate and useful to retrodict from certain physiological laws provided that :

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[1] The concept of primitiveness is used extensively in evolutionary studies but it is rarely defined and hence the way it should be used is often not clearly understood. It presumably means, when a certain species or organ is described as being more primitive than some other species or organ, that they lived or existed at an earlier time than other supposedly genetically related species or organs. The real problem arises, however, as to how to go about applying the term. In those cases where there are fossil remains to help in deciding the temporal appearance of certain species or organs the problem is fairly easy. But when there are no fossil remains the problem is more acute. Often embryological, physiological, or biochemical criteria are used in determining primitiveness. For example, supposing two organs A and B are under discussion and it is asserted that A is more primitive than B. If the embryology of animals X and Y possessed of organs A and B is examined and it is found that in both X and Y organ A appears first but in Y organ A further develops into organ B then this is usually considered good enough reason for considering A as more primitive than B. Where physiological and biochemical criteria are used the simpler processes or reactions, especially if found in morphologically simpler living things, are usually considered the more primitive.

(*a*) historical data are available, or can be found, which support the post-dictions, or
(*b*) the laws are combined with historical data.

To assert, for example, that the ' law of the heart ' (i.e. the greater the initial length of the heart muscle fibres, the more forcible is their contraction) applies to Dinosaur hearts would seem to be rather fruitless. For if someone disagrees with the assertion then there would be no way of settling the issue since there are no historical or other data to support either point of view.

On the other hand, there would seem to be little reason why causal hypotheses explaining how a monkey's arm works should not be applied to the available fossil information on the structure of the arm of *Pithecanthropus*. The results of this post-dicting would give us some idea as to how *Pithecanthropus* moved its limbs and therefore of the range of its possible limb movements. Here at least there are a few historical data to which physiological laws can be related.

The value of evolutionary statements about physiological phenomena is much more doubtful. The basis of these statements seems to be as follows : according to certain assumptions living groups of organisms are arranged in a series with the so-called ' lowest (most primitive) forms ' at the bottom and the ' highest forms ' at the top. Comparisons are then made between certain physiological processes occurring in organisms from each group, from which it is asserted that the physiological processes in question evolved in the direction of the ' lowest ' to the ' highest animal forms '. This method has been used by Smith[1] in a paper 'Water Regulation and its Evolution in the Fishes ', and Redfield[2] in a paper entitled ' The Evolution of the Respiratory Function of the Blood '. In both these papers no historical data are presented sufficient either to formulate a characteristically genetic explanation or to check the extension backwards of any causal explanation of the evolution of these physiological processes. The reason for the lack of historical data is that besides the parts concerned in these processes, kidneys and blood, the physiological processes themselves never survive the fossilising process.

The confusion arises from a method in which a comparison of different forms of life is made and because there is a range from the ' simple ' to the ' complex ' of morphological structure exhibited by the living things, it is assumed that the physiological processes in question evolved from those found in the simplest organisms to those found in the more complex.

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[1] H. W. Smith, *Quart. Rev. Biology*, 1932, 7

[2] A. C. Redfield, *Quart. Rev. Biology*, 1933, 8

As a further example of this method it is worth examining Bishop's paper on 'The Natural History of Nerve Impulse.'[1] In a section on the 'Evolution of Neuronal Complexity' Bishop attempts to arrange into a consistent picture the sequence of stages through which 'the evolving nerve cell may have passed in differentiating the all-or-none impulse out of a generalized functional state of cell surface-polarization'.

He suggests that the properties of activity with respect to which neurones are 'presumed to have become specialized' are as follows: (1) Passive depolarisation, (2) Active depolarisation, (3) Selective sodium permiability, (4) Graded response, (5) All-or-none response.

The actual evidence for this 'presumption' is scanty if not non-existent. He suggests, for example, that the graded response type is more general, as well as more primitive, than the all-or-none response, and that the latter probably developed when an early metazoan became too large, or underwent separation of functionally related parts to too great a distance for graded responses to be effective as a means of communication. Again, if this assertion were challenged and some other hypothesis proposed, it is difficult to see in the circumstances how any historical evidence of these physiological processes could be forthcoming to resolve the conflicting opinions.

At another point Bishop argues that 'in the line of metazoan neurogenic specialization, the first demand must have been for sensory receptor and specialized motor organs, presumably developed as two parts of the same cell as an improvement on the generalized functioning of the first motile protozoan'. The only evidence forthcoming to support this hypothesis is that 'such dual cells still exist in coelenterates unconnected to a nervous system' and 'such cells are still, in a valid if reverse sense, represented in the muscle spindles of vertebrates'. Thus the evidence really consists of retrodictions from generalisations of observation records about present-day living animals. No supporting historical evidence is provided, thus making the original hypothesis a virtually unconfirmable and unfalsifiable speculation.[2]

As to biochemical phenomena, it might be thought that of all the things most difficult to fossilise they are chemical reactions. Yet biochemical historical statements abound, particularly with regard to the so-called Origin of Life and the evolution of biochemical processes in organisms. The method adopted is very similar to that used by comparative physiologists. For example, a comparison is made of special biochemical mechanisms or substances in living organisms from different phyla and where the mechanisms are the same or very similar, a phylogenetic connection is postulated. This method is thus open to the same criticisms that are directed against physiological evolutionary statements, and in fact from the nature of the phenomena dealt with they are even more suspect.

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[1] G. H. Bishop, *Physiological Reviews*, 1956, **136**, 391

[2] Ibid. p. 392

6 *Summary*

(i) The question under discussion is whether science and history can ever be usefully related. It is suggested that unless care is taken such a relation may well lead to confusion, but that in biology at least (and possibly also in sociology and economics) the historical character of part of the subject matter does enable scientific and historical statements to be usefully associated provided certain precautions are taken.

(ii) Confusion may arise if Statements about the Past are used for confirming explanatory hypotheses when the assumptions on which the Interpretive Statements are based are not clearly formulated.

(iii) Biological Statements about the Past may be explained by the kind of explanatory hypothesis found in science provided :

(*a*) the statements are such that they can be usefully generalised ; and

(*b*) the statements are well backed by historical data, and well founded Interpretive statements.

(iv) Causal laws may be formulated, at least in principle, which explain the evolution of characteristics of now extinct animals. But because of the difficulty and sometimes impossibility of obtaining historical data about these various aspects of extinct animals, evolutionary statements about the physiology, biochemistry, behaviour, psychology, and genetics of once living organisms are little more than speculations that cannot be either confirmed or falsified.

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SOCIETAL LAWS [*]

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In an earlier paper I argued that societal facts are not reducible, without remainder, to facts concerning individual behaviour.[1] In short, I argued against one of the basic theses of ' methodological individualism '. However, the issue of whether there are irreducible societal *facts* is not the main problem with which methodological individualism has been concerned. The main problem has been whether or not there are societal *laws* which are irreducible to laws concerning the behaviour of individuals. I would uphold the view that there are, and thus would reject a second thesis of methodological individualism.[2] However, before arguing this point it is necessary to disentangle the problem from some of the misleading issues which have become associated with it. In my opinion, these misleading issues have arisen because it is widely and erroneously assumed that those who reject methodological individualism must accept a position which is termed ' methodological holism '. It is the primary purpose of the present paper to show that there are in fact several alternatives to methodological individualism, and that not all of these alternatives entail an acceptance of ' holism '.[3]

I

Let us briefly review the issue between what has been designated as ' methodological individualism ' and what has been designated as ' methodological holism '.[4]

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[*] Received 29. iv. 57. Read at meetings of the Western Division of the American Philosophical Association, May 1957.

[1] ' Societal Facts ', *British Journal of Sociology*, 1955, 6, 305-317

[2] It will be noted that my view is therefore more radical than that adopted by May Brodbeck in her attack on methodological individualism (' On the Philosophy of the Social Sciences ', *Philosophy of Science*, 1954, 21, 140-156). She rejects the view that there are irreducible societal laws (loc. cit. p. 155).

[3] In a further paper I hope to show how societal laws are to be distinguished from psychological laws, and to argue on the basis of that distinction for the irreducibility of societal laws.

[4] The most important recent formulations of methodological individualism probably are : the articles of Hayek which have been collected in his book *The*

The term 'methodological individualism' seems to have been derived from Schumpeter,[1] and two of its chief exponents are Popper and Hayek. As is well known, the writings in which both of the latter have discussed the methodology of the social sciences have been works which have had a special polemical character: they were not merely discussions of methodology but were attacks on historicism, organicism, and social holism. For this reason, Popper and Hayek have tended to equate a rejection of methodological individualism with the acceptance of methodological holism. This over-simplified classification of alternative theories has now unfortunately become standard.

To illustrate the usual view of the dichotomy between methodological individualism and holism I shall quote a passage from J. W. N. Watkins' defence of methodological individualism:

> If social events like inflation, political revolution, 'the disappearance of the middle classes', etc., are brought about by people, then they must be explained in terms of people; in terms of the situations people confront and the ambitions, fears and ideas which activate them. In short, large-scale *social* phenomena must be accounted for by the situations, dispositions and beliefs of *individuals*. This I call methodological individualism.
>
> You may complain that this is commonsensical and hardly needed saying. The trouble is that some philosophers of history have made the opposite assumption . . . In the secularized version of [their] theory it is the social whole which so determines matters for the individual that he cannot avoid (or would be foolish to try to avoid: the determinism may be a little loose) fulfilling his function within the whole system. On this view, the social behavior of individuals

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Counter-Revolution of Science, Glencoe, 1952; K. R. Popper, 'The Poverty of Historicism', *Economica*, N.S., 1944, **11**, 86-103 and 119-137; **12**, 69-89 (cf. especially p. 80 and p. 88) and his *Open Society and Its Enemies*, London, 1945, Vol. II, Chap. 14; J. W. N. Watkins, 'Ideal Types and Historical Explanation', *British Journal for the Philosophy of Science*, **3**, 22-43, and 'Methodological Individualism: A Reply', *Philosophy of Science*, **22**, 58-62. Also relevant is Isaiah Berlin, *Historical Inevitability*, Oxford, 1954.

Perhaps the most important recent attacks on the principle of methodological individualism are those of Brodbeck (as cited in note 2, p. 211); M. Ginsberg, 'The Individual and Society', in his essays *On the Diversity of Morals*, London, 1957 and 'Factors in Social Change', in *Transactions of the Third World Congress of Sociology*, **1**, 10-19; E. A. Gellner, 'Explanations in History', *Aristotelian Society*, 1956, Suppl. Vol. 30, 157-176; L. J. Goldstein, 'The Inadequacy of the Principle of Methodological Individualism' *Journal of Philosophy*, 1956, **53**, 801-813

[1] Cf. note 50 of Machlup, 'Schumpeter's Economic Methodology', *Review of Economics and Statistics*, 1951, **33**, 145-151

> should be explained in terms of the positions or functions of these individuals and of the laws which govern the system. These laws must be regarded as *sui generis*, applying to the whole as such and not derivable from individualistic principles. This I call methodological holism.[1]

In this passage it is to be noted that a denial of what is defined as methodological individualism is assumed to imply methodological holism. Further, methodological holism is identified with certain forms of the philosophy of history; it is not treated as a methodological principle which might be used by economists, political scientists, anthropologists, or empirical sociologists. Thus it is perhaps not unfair to say that in this passage there is a tendency to assume that the dichotomy between methodological individualism and methodological holism is equivalent to the dichotomy between an empirical explanation of social phenomena and the sort of philosophic interpretation which is characteristic of material philosophies of history.[2] What is more important, however, is that it is assumed that all so-called 'methodological holists' view a social system as an organic whole, the component parts of which are individual human beings. This is not necessarily the case. Some who reject methodological individualism would regard the component parts of a social system as being the institutions which comprise that system.[3] Therefore, *if* they hold that the whole determines the actions of the parts, they are not necessarily arguing that individual human beings are determined by the society as a whole—though they may of course also claim this. And, finally, this passage clearly involves the assumption that if one regards societal laws as being *sui generis* (i.e. as not being in principle reducible to laws concerning individual behaviour) then one must hold that such laws concern the society as a whole. In other words, a rejection of the principle defined as methodological individualism has been assumed to involve an acceptance of the thesis that, whatever societal laws there may be, these laws will concern the functioning of a society treated as an organic whole. It would therefore seem that anyone who wished to reject the *metaphysical* theses of holism in general (e.g. 'the whole is greater than the sum of its parts') would be committed to accepting the *methodological* principle which has been defined as 'methodological individualism'.

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[1] 'Methodological Individualism', loc. cit. p. 58 sq.

[2] This impression is strengthened by the passage omitted in the second paragraph of the above quotation, and by much of the argument used by Hayek. (For a discussion of the nature of material philosophies of history, cf. my article 'Some Neglected Philosophic Problems Regarding History', *Journal of Philosophy*, 1952, **49**, 317-329.)

[3] This is also the view that I would hold. (Cf. 'Societal Facts', loc. cit. 314-315.)

The root error in all this is, I believe, the assumption that all who deny methodological individualism are committed to exactly the same positions as were those nineteenth-century philosophers who attacked the individualistic approach of the eighteenth century. More specifically, the tenets used to characterise 'methodological holism' are precisely those which were held in common by Comte, Hegel, and Marx. This is perhaps understandable in the light of the polemical purposes which we have noted in Popper and Hayek, but it is important to see that a rejection of methodological individualism does not entail an acceptance of an organismic or historicist view of society. This will become apparent in what follows.

I shall proceed by drawing two sets of distinctions concerning law-like statements. By combining these two sets of distinctions into a four-celled table it will be seen that those who accept the belief that there are (or may be) irreducible societal laws can be holding quite diverse views regarding these laws. Leaving aside those law-like statements which seek to reduce societal facts to facts concerning individual behaviour, a societal law could belong to any of the four classes which I shall distinguish. (At certain points it will become apparent that even these classes can be further divided into sub-classes.) It so happens, however, that the theories most frequently discussed by methodological individualists usually fall into only one of these classes. Thus it will be shown that the dichotomy which is usually drawn between methodological individualism and methodological holism is an over-simplified and misleading classification of types of social theory.

2

The first of the distinctions that I wish to draw is between 'a law of functional relation' and 'a law of directional change'. (I shall usually refer to these as 'functional' and as 'directional' laws.) The distinction between these two types of laws, which are sometimes referred to as synchronic and diachronic laws,[1] can perhaps most easily

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[1] Cf. Goldstein's discussion, loc. cit. pp. 808 sqq. (It is to be noted that Goldstein does not deny the applicability of methodological individualism in synchronic studies, though I would do so.)

In an earlier article Edgar Zilsel referred to these as 'temporal laws' and 'simultaneity laws' and attempted to establish that both types are to be found in history (cf. 'Physics and the Problem of Historico-sociological Laws', *Philosophy of Science*, be illustrated through reference to the physical sciences. Boyle's law or Newton's inverse square law would be examples of *functional* laws, while the second law of thermodynamics would be the outstanding example of a *directional* law. In the field of history the distinction would be between what I have elsewhere called ' laws *concerning* history ' and ' laws *of* history '.[1] Marx's theory of the relation between the economic organisation of a society and other institutions in that society (his doctrine of ' the superstructure ') would be a functional law, i.e. a law *concerning* history. On the other hand, one may interpret his view of dialectical development as an attempt to formulate a law stating a necessary pattern of directional change in history, i.e. as a law *of* history.[2]

These two types of law are obviously different. While either would

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1941, **8**, 567-579). It would seem that Zilsel's position with respect to methodological individualism is essentially similar to that held by Brodbeck.

In his *Philosophy of Science*, Madison, 1957, G. Bergmann draws a distinction between ' process laws ' and ' laws of development ' which, I believe, corresponds in many respects to the distinction which I have attempted to draw between functional and directional laws. (He also discriminates between these two types of law and two other types : ' cross-sectional laws ' and ' historical laws '. The first of these is not relevant to our present problem ; the second, as Bergmann shows, is in many cases reducible to a process law.)

[1] ' A Critique of Philosophies of History ', *Journal of Philosophy*, 1948, **45**, 365-378

[2] It appears to me that Marx failed to see the difference between these two types of explanation, and that the second type constitutes a survival of Hegelianism in his social theory. To be sure, there are passages in Marx which would make it appear that the pattern of directional change is merely the result of the forces which he attempts to analyse in terms of his economic theory and of his doctrine of ' the superstructure '. Nevertheless, it does seem equally plausible (at the very least) to maintain that he regarded the pattern which he traced in the history of mankind as having an inherent necessity in it, i.e. that it was ' inevitable ', and was not merely the actual outcome of forces operating from moment to moment. (Even on the latter interpretation of Marx, there are two possible alternative interpretations : (*a*) that the inevitability rests on the necessary pattern of development in the means of production, particularly the technology ; (*b*) that the inevitability of the pattern is an expression of the ultimate dialectical process in all reality.)

A somewhat similar confusion between laws *of* history and laws *concerning* history is present in Toynbee. His concept of ' challenge and response ' purports to throw light on the forces at work at each stage in the course of a civilisation's history, but the pattern of change which he traces seems to assume the shape of a quasi-inevitable directional tendency. In other words, the concept of ' challenge and response ' performs the same sort of function as a law *concerning* history would perform in a scientifically oriented theory, while his construction of the histories of civilisation in terms of their stages is an attempt to show that there is an inherent tendency for the course of history to follow a definite pattern of development.

make prediction possible if we possessed adequate knowledge of the initial and boundary conditions of the state of affairs to which the law was to be applied, the first type of law (a functional law) would only enable us to predict immediately subsequent events, and each further prediction would have to rest upon knowledge of the initial and boundary conditions obtaining at that time. The second type of law (a directional law) would not demand a knowledge of subsequent initial conditions (though it would assume stability of boundary conditions), for if there were a law of directional change which could be discovered in any segment of history we could extrapolate to the past and to the future without needing to gather knowledge of the initial conditions obtaining at each successive point in the historical process.[1]

We now come to the second distinction which I wish to draw : a distinction between what I shall designate as '*abstractive* laws' and what I shall designate as '*global* laws'.

In an abstractive law the attempt is made to state a relation between specific aspects or components which are present in a state of affairs, and to state this relation in such a way that it will be applicable in all cases in which these particular aspects or components are present. In formulating such a law, the specific nature of the state of affairs in which these elements are to be found does not enter into the law itself, but is only considered with respect to the initial and boundary conditions which must be taken into account in applying the law.

On the other hand, it is possible to regard some entities in terms of their global properties, to consider them as unitary systems, or wholes. And it may be possible that when we so regard them we can formulate law-like statements concerning changes in their global properties, or concerning relationships between the nature of the system as a whole and the manner in which its component parts behave. In stating such a law we are considering the system as a system, and a reference to the properties of the system is included in the law which we formulate.

Now, in order to avoid an unnecessary misunderstanding, it must immediately be pointed out that the acceptance of a global law (or of the possibility that there are global laws) does not commit one to the position of 'emergence', or to any form of 'holism'. Such laws might be held to be derivative from laws concerning the component aspects of the system, and thus be reducible to abstractive laws. To be sure, one might not so regard them, and in that case one would in all likelihood be accepting the position usually designated as ' holism '. Yet even this is not necessary. For example, if one were to hold that there are laws concerning the relations between one component in a certain type of system, and the nature of any such system considered as a whole, and if one were also to hold that it is this component which determines the properties of the whole, then one would be formulating a law concerning the global properties of a system, and yet not be a ' holist ' in the most usual sense of that term : it would be a part which determines the nature of the whole, not the whole which determines the nature of its parts.

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[1] Cf. the method of Henry Adams. It is to be noted that in those cases in which a directional law is derived from metaphysical principles (rather than being empirically derived), it is not even thought to be necessary to assume stability in the boundary conditions for a society.

This last warning may help to elucidate the distinction which I have sought to draw between abstractive and global laws. The distinction is not one between non-holism and holism, but between laws which are formulated in terms of particular aspects or components which have been abstracted from a concrete state of affairs, and laws which are formulated in terms of the nature of particular types of system. In other words, there is a difference in what the laws are *about*. Abstractive laws are about the relationships between two aspects or components that occur in a variety of different concrete situations ; the nature of the situations in which these aspects are embedded constitute the initial and boundary conditions which must be taken into account in applying the law. Global laws, on the other hand, are about the properties of systems, attempting to show how these systems change over time, or how the system as a whole is related to its component parts.

3

We have now drawn two distinctions : first, a distinction between laws concerning functional relations and laws concerning directional change ; second, a distinction between abstractive laws and global laws. These distinctions engender four possibilities concerning law-like statements, and if we now examine the theories of those who have rejected the principle of methodological individualism we shall find that social theorists have attempted to formulate law-like statements of each of the four types. However, methodological individualism has been almost solely concerned with a criticism of the attempt to find *global laws of directional change*. It has in fact tended to identify the view that there are irreducible societal laws with a belief in laws of this type. If we now briefly examine each of the four types we shall be in a position to see to what extent, if at all, each may properly be regarded as holistic.

(i) Let us first examine the view that there are law-like statements which are both functional and global. Such laws would relate the global properties of a social system as a whole to one or more of its component parts, i.e. to its specific institutions or sanctioned usages.

As we have already noted, there are two main ways of proceeding in attempting to establish laws of this general type. On the one hand, we may regard the global properties as determinants of the properties possessed by component parts of the system. This is the case in Ruth Benedict's descriptive analysis of particular patterns of culture ; it is also the case in Radcliffe-Brown's form of Functionalism in which the need for self-maintenance in a society considered as a whole determines specific usages, such as the punishment of criminals or funeral ceremonies.[1] Marx, on the other hand, would derive at least certain of the global properties of a social system (e.g. the defining characteristics of ' feudalism ' or ' capitalism ') from one specific component within that system, viz. the means of production. Others might seek to find a composition law by means of which, given the properties of two or more component parts of the system, the global properties of the system as a whole could be derived. All of these types of law would be instances of an attempt to state global laws of the functional type, i.e. laws which involve the relation of its components to the concrete nature of a system considered as a whole. I believe that it is obvious that not all of these attempts would be ' holistic ' in the same sense.

(ii) It would also be possible to attempt to establish laws of directional change concerning global properties. Such laws would not be seeking to relate the properties of a system as a whole to the nature of one or more of its component parts, but would attempt to formulate a law-like statement concerning the successive states of a system. In other words, a law of this type would be a statement concerning a pattern of directional change in a social system considered as a whole, e.g. that there exists a specific sort of uni-directional development, or

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[1] Cf. Radcliffe-Brown, ' On the Concept of Function in Social Science ', *American Anthropologist*, 1935, **37**, 394-402. (The specific illustration used is to be found on p. 396.) Also, ' On Social Structure ', *Journal of the Royal Anthropological Institute*, 1940, **70**, 1-12.

It is to be noted that Radcliffe-Brown's form of Functionalism is to be distinguished from that held by Malinowski (after 1926). On this distinction, cf. Radcliffe-Brown, ' A Note on Functional Anthropology ', *Man*, 1946, **46**, §30, and ' Functionalism : A Protest ', *American Anthropologist*, 1949, **51**, 320-323.

a cyclic flow, in the over-all aspects of a society. Such a law, of course, would not be intended to be merely a description of what has taken place in one social system during a restricted time interval, but would be held to be applicable at all times and with respect to all societies,[1] or with respect to all segments of one all-embracing historical process, viz. to the history of mankind as a whole.

A theory of history which is based on the belief in a law of this type would regard the over-all changes which took place as being ' inevitable '. Furthermore, such theories usually regard the directional law which they seek to establish as being an ultimate law, i.e. one not deducible from functional laws concerning the components of the systems. Where this is the case, we may legitimately speak of ' holism '. However, we must note that such a law might *not* be treated as ultimate. For example, it might not be wholly misleading[2] to rephrase Marx's doctrine concerning historical inevitability to make it appear that the law of directional change in history is a consequence of two other laws : a functional law asserting that the global properties of a social system are determined by the means of production, and a directional law concerning changes in this specific component. Such a position would still maintain the thesis that there is a law descriptive of the direction of social change in all social systems, and it would therefore espouse the doctrine of historical inevitability, but it would not be an example of ' holism '.

The above suggestion is not intended to be a contribution to the exegesis of Marx, but is included for two other reasons. First, it should serve to suggest that not all doctrines of historical inevitability are ' holistic ', or (at least) equally ' holistic.'[3] Second, it serves to introduce the third general type of law with which we are concerned : the attempt to find a directional law which is abstractive in the sense that it is concerned with changes occurring in one component of a social system, and not with the social system considered as a whole.

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[1] Of course, this may be limited to societies of a given type, e.g. to ' civilisations ' but not to ' primitive societies '.

[2] Cf. note 2, p. 215

[3] It should also be noted that not all examples of holism in the philosophy of history accept the thesis of historical inevitability, if by ' inevitability ' is meant that each stage in the development follows necessarily from the preceding stage. In Herder's philosophy of history, for example, the element of necessity is strongly stressed in each part of the process, but the process as a whole is not viewed as proceeding through an over-riding necessity. (For example, cf. *Ideen zur Philosophie der Geschichte der Menschheit*, 13th Book, 7th Chapter.)

(iii) Attempts to formulate laws of directional change concerning specific institutions, regardless of the societies in which they are embedded, have been very prevalent in anthropological and sociological theory. Among the many examples it is only necessary to call to mind theories of the necessary stages in religious development, or in marriage systems; attempts to formulate a law concerning the tendency of language to change from a more complex to a simpler structure, or of the arts to develop from the abstract-decorative toward the representational pole (or *vice versa*). There have, of course, also been attempts to trace cyclical changes (and to formulate laws concerning the sequence of such changes) in political forms or in styles of art. Whether these directional laws regarding specific institutions have been unidirectional or cyclic, those who have formulated them have not usually viewed them as derivative from abstractive functional laws.[1] Rather, they have generally (but not always) regarded them as derivative from other laws of directional change. These more basic directional laws have usually been of either two kinds: laws concerning the direction of change in the total social system (i.e. directional laws of the global type) or laws concerning the changes in the nature of the human mind. By holding that there is a necessary direction of change in a society as a whole, and by holding that form of global law which states that the whole is so related to its parts that the parts are determined by the whole, a law of change concerning a specific institution follows. Or by holding that there is a particular pattern of growth in the capacities of the human mind, and by making the assumption that each institution will go through stages which reflect this growth, a law of change in a specific institution could be derived. These two alternative ways of deriving a law of directional change in a specific institution from some more ultimate law of change are not incompatible. They are not incompatible since it is possible to hold (and has often been held, e.g. by Comte and, in a sense, by Hegel) that the law of development which applies to the properties of society considered as one systematic whole, is a reflection of the development of the human mind or spirit. This should be recalled by those who tend to regard holist views of the structure of society as being primarily due to the tendency to regard societal facts as different from facts concerning human thought and action.

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[1] This is to be expected, since the attempt to formulate such a directional law of change with respect to a specific institution involves an isolation of that component from the other societal components which are contemporaneous with it.

(iv) The fourth possible type of societal law which follows from our distinctions would consist in the attempt to state functional laws of the abstractive type. Among the examples of attempts to formulate such laws we may cite the following : statements concerning relationships between modes of production and marriage systems ; between size of population and political organisation ; between forms of economic organisation and political organisation ; or, to cite a classic study of Tylor's[1] (which has been amplified and elaborated by Murdock in his *Social Structure*) between certain specific aspects of marriage systems, e.g. rules of residence and rules of descent.

It is to be noted that laws of this abstractive-functional type are different in aim from the laws of the other three types.[2] In being functional laws, they do not assume that there is any necessary direction of historical change, either within specific institutions or within a society considered as a whole. Furthermore, in being abstractive they seek to explain such changes as do occur in terms of the successive initial and boundary conditions which obtain at specific points in time, and do not assume that these conditions must be identical in all societies. In being abstractive they also remain uncommitted on the question of whether any particular society (or every society) can be regarded as a single organic whole ; it becomes the task of empirical investigation to determine to what extent the various aspects of a given state of affairs *are* interrelated. Thus, those who hold that there are (or may be) abstractive-functional societal laws are neither committed to historicism nor to organicism, both of which have usually been held to be consequences of the rejection of methodological individualism.

4

Without entering into the empirical and methodological issues which are involved, I should like to state that I believe there are important reasons for doubting that we shall find irreducible societal laws of the first three types.[1] Therefore I venture the suggestion that *if* there are empirically derivable laws concerning social phenomena, and if these cannot all be reduced to laws concerning individual behaviour, those which cannot be so reduced will be abstractive laws concerning functional relations between specific types of societal facts. Whether such laws have been found, or whether we have reason to believe that they may be found, is not the question which I have proposed for this discussion. However, before closing, I should like briefly to indicate the various possible relationships which might obtain between psychological laws concerning the behaviour of individuals and laws which attempt to state functional relationships between specific aspects of social structures.

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[1] ' On a Method of Investigating the Development of Institutions ; Applied to Laws of Marriage and Descent ', *Journal of the Royal Anthropological Institute*, 1889, **18**, 245-269.

[2] To be sure, the purpose for which Tylor, as a social evolutionist, used his data concerning functional relations was primarily for the reconstruction of the evolution of marriage systems. It is also to be admitted that his explanations of *why* ' adhesions ' took place were often couched in psychological terms. However, he does not appear to have believed that such psychological explanations were alone sufficient to account for the facts (cf. loc. cit. p. 248).

If one assumes that there are (or may be) laws which accurately express the functional relations between two or more specific types of fact in all societies (or in all societies of a specific type), it could be the case that these laws follow deductively from laws of individual behaviour when one takes into account the conditions obtaining in the societies in question. This is the view held by methodological individualism which rejects *irreducible* societal laws.

If, however, one believes that there are irreducible societal laws, two positions still remain open to one. One *might* hold that such laws are themselves sufficient (granted the initial and boundary conditions) to explain all that occurs in societies. To this position methodological individualists would doubtless be no less opposed than they are to a holistic view, since this view would also render human choice and human action nugatory in the realm of social affairs. On the other hand, one might hold that an adequate explanation of social phenomena would have to use *both* psychological laws and societal laws, and that neither of these types of law is reducible to the other.

There would, I submit, be nothing mysterious in such a claim.[2] When, for example, we wish to explain a concrete phenomenon of social history such as the failure of a particular soil conservation programme, we need to employ psychological generalisations concerning human behaviour, but we also need to employ generalisations drawn from the physical sciences concerning the effects of the conservation steps which were taken. In such cases of interaction between men and their physical environment no-one, I should suppose, would challenge the belief that an adequate explanation of the series of events would demand the use of laws belonging to different sciences, as well as demanding a knowledge of the initial conditions relevant to the application of each set of laws. Further, no-one, I should suppose, would argue that one set of these laws must be reducible to the other in order that we should be able to use both in explaining this concrete event. And I see no reason why an analogous situation could not obtain with respect to the explanation of social phenomena, i.e. that in such explanations we may need to employ both psychological and societal laws. A belief of this type would not entail the acceptance of historical inevitability, nor would it entail any form of holism. Finally we may note that such a view would not necessarily entail a rejection of the thesis that psychological laws are *always* relevant to the explanation of social phenomena. It could accept this thesis and yet hold that *in some cases*[1] a knowledge of the initial conditions under which individuals act, and a knowledge of the laws of individual behaviour, is not adequate to explain the outcome of their actions : for this one must also employ abstractive-functional generalisations concerning societal facts.

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[1] Some reasons for doubting each of these three types are implicit in my earlier article, ' A Critique of Philosophies of History ', *Journal of Philosophy*, 1948, **45**, 365-378. I shall discuss the issue in detail in a future article.

[2] I believe that Gellner is correct in thinking that many methodological individualists *would* think this ' mysterious '. (Cf. Gellner, loc. cit. pp. 167-168.)

The purpose of this paper has been limited to pointing out that a rejection of methodological individualism is compatible with a number of diverse views concerning the nature of societal laws. Therefore the simple dichotomy of methodological individualism *or* methodological holism stands in need of drastic revision. It seems to me that the classification which I have offered may also help to point out frequently unnoticed similarities and diversities among social theorists, thus making an analysis of their theories somewhat more manageable. And if, as I believe, there are good empirical and methodological reasons to doubt the feasibility of establishing either abstractive or global laws of directional change, or global laws of a functional sort,[2] then the issue of whether there are irreducible societal laws can be confined to one set of claims : that there are (or are not) some irreducible laws governing the functional relationships of specific aspects or components in societal life. The establishment of such laws would not demand that we accept the thesis of historical inevitability, nor the political and moral implications of either historicism or organicism. Nor would it commit us to either the metaphysics, or the explanatory methods, of holism.

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[1] I should not myself be inclined to accept the more radical form of the doctrine here under consideration, viz. that in *all* cases this is true.

[2] I am here only referring to laws of this type which are claimed to be ' ultimate ', i.e. not reducible to abstractive-functional laws.

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HOW ECONOMIC THEORY MAY MISLEAD *

ANNE MARTIN

1 *Introduction*

ONE of the most distinguished living theoretical economists, Mrs Joan Robinson, once wrote :

> the student's heart sinks when he is presented with a book on the scope and method of his subject. Let me make a start, he begs, and I will find out the scope and method as I go along. And the student is perfectly right. For a serious subject, in the academic sense, is neither more nor less than its own technique.[1]

It is, of course, true that we cannot talk usefully about the method of any subject until we have either practised it or observed its practice very closely. But it is only too possible for the practitioners of any 'serious subject' to fall into persistent errors, and for these errors to become sanctified by the passage of time and the prestige of their perpetrators. Workers in all the social sciences have, at present, a chronic tendency to pull their subjects up by the roots to observe their growth ; but this is the reflection of a perfectly honourable *malaise*.

The public demands from the economist that he should both explain, like the physiologist, and prescribe and cure like the physician. And many economists are themselves unhappy that they have not made more progress with the latter task. Even today, when the work of Keynes has seeped into the administrative consciousness in many countries, they would despondently agree with Wootton, 'I can find very little evidence that the ability of economic science to interpret concrete situations usefully has advanced appreciably during the past hundred years '.[2]

'To interpret concrete situations usefully' presumably means to make correct predictions from them, and, more importantly, to bring

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* Received 11. i. 57. The stimulation for this article came largely from reading T. W. Hutchison, *The Significance and Basic Postulates of Economic Theory*, London, 1938.

[1] Joan Robinson, *Economics is a serious subject*, Cambridge, 1932, p. 3

[2] Barbara Wootton, *Lament for Economics*, London, 1938, p. 67

about desirable, and prevent undesirable, states of affairs. Although the ability to control implies the ability to predict, the converse does not hold. The astronomer can predict planetary movements without any hope of controlling them, and in some cases the social scientist may be in an analogous position (we might be able to predict excellently when a totalitarian régime is due for a phase of greater or less repression without being able to do anything about it). But failure to control may also, and more interestingly, be due to a failure of theory. We might, for instance, be able to predict that some degree of inflation is likely to continue in Britain for the next decade, because we know the constellation of economic conditions which normally breeds inflation. But we might not know enough about the interaction of the relevant factors, or their relative importance, to be able to prevent inflation occurring. Incidentally, it is because *control* of economic and social situations is so important that the social scientist should refuse to abandon the concept of ' cause ', however often he is told that physicists now prefer to talk of ' function '. We should reject Hutchison's austerity in saying,[1] ' We certainly do not volunteer . . . to give any other meaning to concepts like " the cause of a trade depression " than, simply " certain events immediately preceding or accompanying a trade depression " '. If, for instance, we found that we could cure inflation by restricting consumer credit, it would be reasonable to say that we had discovered the *cause* of at least some inflations. A knowledge of causation sufficient for effective control does not, of course, guarantee *explanation*. A child knows that a dropped toy falls to the ground long before he has heard of gravity. The economist will not be satisfied until he can explain as well as predict, that is, until he can account for certain events in terms of others, antecedent or concurrent, and show fairly exhaustively *how* they bring about the result. We use the word ' function ' to indicate the belief that there *is* a ' how ' to be discovered. As long as we are unsure whether two events, statistically correlated, such as the increase in the Irish pig population and the drift off the land in Denmark, are not in purely fortuitous relation, we are careful not to describe one as a ' function ' of the other.

Before turning to the method of economics, let us briefly consider its subject matter. The definitions of Marshall and of Robbins are both widely quoted : ' a study of mankind in the ordinary business of life ',[2] and ' the science which studies human behaviour as a relationship between ends and scarce means which have alternative uses '.[1] Both seem rather too wide, to cover considerably more than the field in which economists, qua economists, in fact consider themselves qualified to work. We can illustrate from a discussion of Boulding's [2] in which he expands Robbins' definition, calling economics 'a generalised theory of choice'.

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[1] Op. cit. p. 72

[2] A. Marshall, *Principles of Economics*, London, 1907, p. 1

> Even in Adam Smith we find the explanation of wage differentials in what might be called the non-monetary advantages of the various occupations, and little more than a hundred years later we find Wicksteed illustrating the principles of value theory with reference to the problem of how high a cliff one would dive off to save a mother-in-law, or how much family prayers should be shortened to speed a parting guest to the train.

With all respect to a great economist, Wicksteed's examples seem unconvincing and likely to mislead. In going beyond those things 'which can be bought into relation with the measuring-rod of money',[3] he takes examples of choice which are spuriously quantitative: spuriously, because in this sort of ethical problem measurable magnitudes are, in fact, rarely relevant. There are, of course, many situations where one can say of someone, 'he weighed up the factors on both sides and came to a decision', but the economist is in no way specially qualified to describe this process. Some psychologists use the phrase 'mental economy' in describing the process of choice, but they do not thereby appeal for the economist's help, any more than does the botanist who talks of 'plant economy'. The non-monetary advantages that enter into wage differentials do, of course, concern the economist closely, but specialisation in the social sciences is increasing and the economist may now perhaps rely on workers in other branches of the social sciences to present him with information on this sort of topic. Mill gives us as good a brief description as any of the scope of economics [4]:

> the science which traces the laws of such of the phenomena of society as arise from the combined operations of mankind for the production of wealth, in so far as these phenomena are not modified by the pursuit of any other object.

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[1] L. Robbins, *An Essay on the Nature and Significance of Economic Science*, London, 1935, p. 16

[2] K. E. Boulding, 'Is Economics Necessary?', *The Scientific Monthly*, 1949, **68**, 236

[3] A. C. Pigou, *The Economics of Welfare*, London, 1932, p. 11

[4] J. S. Mill, *Essays on some Unsettled Questions of Political Economy*, London, 1874, p. 140

In short, the material of economic study is given by the two facts of change in the ownership, production, and consumption of material wealth—since, if change is not possible, neither is choice—and of the desire of individuals to increase their material wealth. We might, then, wish to omit from Mill's definition reference to 'the combined operations of mankind', since the factors of change—say, a technological brain-wave, or the appearance of a shoal of herring—and of 'materialism', in this special sense, could be present in a Crusoe economy.

2 *Common Sources of Confusion*

> Nothing but unreality can come of [political economy] till we know when and how far its first assertions are true in matter of fact, and when and how far they are not.[1]

Economists have often been regrettably unclear as to whether their 'first assertions' are *intended* to be true in matter of fact. Oskar Lange clearly thinks they are, calling the basic postulates of economic theory 'approximative generalisations of empirical observations'.[2] It is not clear, however, whether he thinks that the postulates are supposed to be directly testable, or whether they are selected because they are likely to lead to other testable hypotheses. Some equally eminent economists appear to have thought that they are chosen for their capacity to spawn logical consequences which together form an intricate and aesthetically satisfying pattern, regardless of whether or not any empirical test can be applied to them and of whether such a test usually strengthens or weakens them.[3] Economists, like anyone else, may, on occasion, fall into every kind of logical error, but there are four to which they have been particularly prone and which, between them, mostly account for the inability to agree about the status of their Postulates, or Laws.

The first error consists of treating an analytic proposition as if it were synthetic.[4] As an example, let us take the proposition that a

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[1] W. Bagehot, *Economic Studies*, London, 1908, p. 94

[2] O. Lange, 'The Scope and Method of Economics', *Review of Economic Studies*, 13, 1945, 21. But compare note 1, p. 229.

[3] For example, Robinson writes: 'unless economics is content to remain for ever in the age of Alchemy, it must resolutely turn its back on the pursuit of gold, however precious it may be to the human welfare, and embark upon the path of an austere and disinterested search, not for "the Truth", but for a single self-consistent system of ideas.' Op. cit. p. 4.

[4] Throughout this paper I define a synthetic proposition as one that could be falsified by empirical test and an analytic proposition as one that could not.

'rational' entrepreneur will always try to equate marginal cost and marginal revenue. This is true in economic theory by virtue of the definition of 'rational entrepreneur' as one who tries to maximise monetary gains in the present or near future.[1] It was Robinson who pointed out[2] that economists are always in danger of giving their purely formal statements a misleadingly empirical appearance just because they have adopted a technical vocabulary to only a limited extent. Their theoretical constructs bear the name 'firm', 'entrepreneur', 'consumer', 'market', so that it is only too easy to understand a proposition where the word 'entrepreneur' should appear in inverted commas, as being an empirical statement about the actual behaviour of real-life entrepreneurs. And this has constantly been done, though by no means by all economists. Bagehot, for instance, wrote quite uncompromisingly,[3] 'English Political Economists are not speaking of real men, but of imaginary ones; not of men as we see them, but of men as it is convenient to us to suppose they are'. This sort of error is particularly insidious because the author of the proposition can so easily himself believe it to be synthetic. How an entrepreneur behaves in adjusting his costs to his revenue is just the sort of thing one would expect to be able to discover by careful observation and enquiry. So that propositions of this sort give the false impression that empirical research, which badly needs to be done, *has* been done.

Our example has introduced the 'rational entrepreneur' and the economist's special use of the word 'rational' has itself been responsible for considerable confusion. To quote Robinson again, 'the fundamental assumption of economic analysis is that every individual acts in a sensible manner, and it is sensible for the individual to balance marginal cost against marginal gain.'[4] This, or something like it, has indeed been a fundamental assumption of economics, as important as is the principle of the rectilinear propagation of light to geometrical optics.[5] In each case, before we had the principle we could not have the science. Robinson's formulation, however, begs the question of what we ordinarily mean by sensible conduct, and she would perhaps have done better to write 'that every individual acts in a sensible manner and *that* it is sensible for the individual . . .', making it clear that this definition of ' sensible ' is part of our assumption or, for that matter, enabling us to drop the word ' sensible ' altogether. The earlier characterisations of ' economic man ' said less and were to that extent closer to reality. ' Man is a being who is determined, by the necessity of his nature, to prefer a greater portion of wealth to a smaller in all cases '.[1] Mill's economic man has only to know what he wants, or to act as if he knows : Robinson's, however, has also to know how to get it and to apply the appropriate method consistently. We are faced here with a serious dilemma : if we assume only that our economic subjects try, roughly, to make the best material bargain they can, we have remained reasonably close to what everyday observation tells us, but we will find our economic theory very difficult to construct. If we assume, further, that they know the theoretical principles on which they should act so as to make the best bargains, and have the knowledge and capacity to apply the principles, we make our theoretical problems much more tractable, but get alarmingly far from reality. If we make the latter assumption we are bound to hold that the consumer apportions his expenditure so that he would not wish to alter it by any practicable amount so long as his income, tastes, and the prices and qualities of all goods remain constant : that the entrepreneur both intends to equate marginal costs and marginal revenue and succeeds in so doing ; and that the investor always finds the most lucrative channels for his investment. Every first-year student, who is at all interested, boggles at just this point. People aren't at all like this, he says ; not only do they seldom know enough but, most of the time—particularly if we are considering consumers rather than entrepreneurs—they do not even care enough to try. If economic theory consists in mapping the consequences of behaving in a way in which in fact no one consistently behaves, and many people seldom behave, can it be of any use ? That a principle helps us to think in an orderly fashion is not enough : we can be orderly and wrong. Many fruitless and inexpert debates on empirical psychology would have been saved if economists had always distinguished clearly between useful methodological devices and the results of empirical observation. This particular assumption is useful if we can use it as a basis for an orderly description of economic phenomena and *eventual explanation.* And it fulfils these criteria if people commonly do act ' rationally ' in recognisable situations. If so, then at least we have advanced our capacity to think about *these* situations. Our assumption probably does in fact, fulfil

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[1] As Lange puts it, 'a unit of economic decision is said to act rationally when its objective is the maximization of a magnitude. Firms thus act rationally, by definition.' Op. cit. p. 30.

[2] Op. cit. pp. 12-13

[3] Op. cit. p. 7

[4] Joan Robinson, *Economics of Imperfect Competition*, London, 1933, pp. 241-242

[5] Toulmin, *The Philosophy of Science*, London, 1953, p. 83

[1] Mill, op. cit. pp. 138-139

our criteria and, more, the relevant situations, e.g. stock market behaviour, decisions on investment, production, and price setting, are of undeniable importance in the complex of the material welfare of modern societies.

A relatively mild reform in terminology might, however, help economists to avoid certain confusions into which their particular concept of ' the rational man ' has led them. I would suggest using the more neutral adjective ' economic ' to describe people who are trying to make the best material bargain open to them. (I say ' make the best bargain ' rather than ' maximise gain ' in deference to Lamont's point[1] that the study of economics normally presupposes a known legal and institutional framework, which sets the terms of the possible bargains, and cannot throw much light on situations in which force and fraud are dominant.) The words ' rational ' and ' sensible ' would then retain their ordinary usage meaning, roughly, ' acting prudently on the information available '. Economists already have a more technical term, viz. ' economically efficient ' to describe a situation where the best bargain actually is made, marginal returns do prove to have equalled marginal costs, and so on. This use would be particularly helpful in reminding us that ' economic efficiency ' is essentially an ' ex post ' concept, while ' economic ' or ' rational ' action is ' ex ante '. As an example of the consequences of confusing these logically distinct viewpoints, I quote from a recently published paper[2] ;

> It will be taken that capital newly available for use is being apportioned in an optimum way if the marginal return on capital is in every use, in every business, in every industry the same. ' Marginal return on capital ' is to mean the *estimated* increase in value, represented by the higher output *expected* to result from an increase of one unit in the amount of new capital to be applied to a particular use. (Italics mine.)

And again :

> If, owing to the high level of risk attending the *estimated* performance of the business (or indeed, for any other reason) the return on the capital *estimated* in this way falls below what is *estimated* for it in some alternative use, and if, consequently, the capital is withheld, then that is as it should be. (Italics mine.)

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[1] W. D. Lamont, ' The Concept of Welfare in Economics ', *Proceedings of the Aristotelian Society*, Sup. Vol. 32, 1953, 166–167

[2] R. H. Tuck, ' A Reconsideration of the Theory of Agricultural Credit ', *Journal of Agricultural Economics*, 1956, **12**, 21, 22

It is 'as it should be', surely, only if expectations subsequently turn out to have been right. Similarly, we would only want to talk of the optimal apportionment of capital when, *after the event*, we decide that it could not have been improved upon. To apportion capital so as to equalise marginal return, in our author's sense, is of course *rational*, but economically efficient only if, by luck or judgment, our expectations are fulfilled. It is, of course, always difficult and often impossible to know whether we have attained an optimal position, since we often lack full knowledge of the alternative possibilities that were open to us. But optimum allocation, or economic efficiency, still remains a useful concept, a Kantian 'regulative idea'. Max Weber illustrates well by an analogy[1]:

> In order, for example, to understand how a war is conducted, it is necessary to imagine an ideal commander-in-chief for each side. . . . Each of these commanders must know the total fighting resources of each side and all the possibilities arising therefrom. . . . On the basis of this knowledge they must act entirely without error and in a logically 'perfect' way. For only then can the consequences of the fact that the real commanders neither had the knowledge, nor were free from error, and that they were not purely rational thinking machines, be unambiguously established. The 'ideal' constructions of rigorous and errorless rational conduct which we find in pure economic theory have exactly the same significance.

The second type of muddle into which economists commonly lead themselves and others is to start with a plainly synthetic proposition, find it inconveniently often falsified, and restate it so as to make it, in effect, analytic. The laws of Supply and Demand, as usually stated, provide a good example of this, e.g. 'a rise in price tends, sooner or later, to decrease demand and to increase supply'.[2] In fact, of course, as in Britain today, we often find that a rise in price of a particular commodity is followed by an *increased* demand for it; or, as in the early 1930's, that a fall in price is followed by a *decreased* demand. Rather than say that their account is often false, however, economists have adopted the procedure of either saying 'a rise in price *tends* . . .' or of introducing the phrase '*ceteris paribus*'.[3] This effectively makes the proposition unfalsifiable since, in the first case, we can always exculpate the law by saying 'we only claimed to show a tendency and contrary tendencies must have been at work'; and, in the second case, by saying 'other things didn't remain the same'. Either procedure leaves us without clearly defined conditions under which the law does not hold. Laws in other sciences frequently have a '*ceteris paribus*' clause, often implicit, but it is usually possible to say just what is included in the 'other things' and what is not. Boyle's Law, for instance, is not universally true and physicists in fact understand it as applying only under certain conditions, e.g. of temperature, *which they are able to state*. In economics, however, the clause usually performs an obscurantist function, and may do so in either of two ways: either the factors to be included in the 'other things' are not specified, so that the supposition of any change whatever in the economic world saves the law; or else the factors that are listed, such as consumers' tastes, real incomes, availability of substitute commodities, are just those whose change is extremely difficult, or impossible, to observe in isolation. This makes many economic statements look like statements about the other side of the moon.

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[1] M. Weber, *The Methodology of the Social Sciences* (Tr.), Glencoe, 1944, p. 42

[2] Henderson, *Supply and Demand*, Cambridge, 1932, p. 19

[3] Cf. Hutchison, op. cit. pp. 40-46

The third characteristic confusion is a failure to distinguish between rules which are essentially guides to the construction of economic *theory* and rules which are guides for economic *policy*: between rules which help us to describe, and those which help us to chide reality. It is hard to believe that this confusion has not sometimes been wilful. It is sometimes exemplified in an ambiguous use of the word 'ideal'. The state of perfect competition, for example, is often thus qualified and it is not always clear whether we should understand this as meaning (i) a simplified model, like a frictionless world in mechanics, which is convenient as a starting-point for explanation, or (ii) perfect, in some normative sense. We may agree that economic laws are 'directives for the investigator to find his way about in reality',[1] or heuristic postulates;[2] but it does not follow that they should be taken as imperatives of economic *behaviour*, although this view has been held by distinguished economists. Knight, for instance, writes, 'it is the function of economic principles to describe the ideal, not the reality; and it is an ideal for social policy and not merely the pattern to which the individual wishes and tries to conform'.[3] Von Mises puts this viewpoint more extremely when he writes ‘ knowledge of Political Economy leads necessarily to Liberalism ’.[1] Whatever we may think of the implications of this view for economic policy, it can only have a harmful effect on the method of economic study. If we treat economic laws as imperatives in *this* sense, we lose any possibility of testing the propositions we derive from them. And as long as a study results in no propositions which can at least in principle be subjected to empirical test and thereby falsified or strengthened, it can only be pseudo-science. Knight writes :[2] ‘ it is not conceivably possible to verify any proposition about economic theory by any empirical procedure. . . . Economy involves an intention or intended result which is not amenable to observation in any admissible use of that term.’ If the propositions of economic theory were really about intentions, it might perhaps seem that introspection, or *verstehende*, would be one method of testing them : although it would still be open to doubt whether it was the best way. The task of economics, however, is to explain the economic behaviour of *aggregates* of individuals, and its results, and for this purpose it is not necessary to explain the psychological antecedents of that behaviour. ‘ That one’s “ ultimate ” data should be statistical can no longer be considered a shocking idea. We can, in this respect, draw a *good* analogy with physics, in which science the behaviour of electrons is now held to be indeterminate.’[3] So that there is no need to discuss here the nature of introspection. We need only maintain that no proposition can be directly useful in explanation unless it can conceivably be falsified. This is not to say that the methods of strengthening or falsifying economic propositions are simple : only that we should not so frame our propositions as to exclude, from the start, any possibility of testing them.

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[1] Moritz Schlick, quoted by Toulmin, op. cit. p. 92

[2] Cf. Kaufmann, 'On the Postulates of Economic Theory', *Social Research*, 1942, **9**, 388

[3] F. M. Knight, 'The Significance and Basic Postulates of Economic Theory: A Rejoinder', *Journal of Political Economy*, 1941, **49**, 752

The conclusions of Knight and von Mises may perhaps depend on a fourth type of confusion ; a failure to distinguish between two types of hypothetical imperative, that is, statements of the type, ‘ if you want x, you must do y ’. We might divide such statements into two classes, the analytic-hypothetical and the synthetic-hypothetical. If, for example, we were to make freedom from internal parasites part of our definition of ‘ health ’, then the imperative, ‘ if you want to be healthy, you must avoid ingesting parasites ’ is an example of the first type.

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[1] Quoted by Hutchison, op. cit. p. 184

[2] Knight, ‘ “ What is Truth ”, in Economics ? ’, *The Journal of Political Economy*, 1940, 48, 15

[3] I. M. D. Little, *A Critique of Welfare Economics*, Oxford, 1950, p. 52

'If you want to be healthy, you must boil your drinking water' is an example of the second type, since its validity depends on the testable proposition that unboiled water is likely to contain live parasites. The hypothetical imperatives of economics (particularly Welfare Economics) can usefully be distinguished in this way. Pigou's basic proposition,[1] for instance, is that in order to maximise the national dividend, taken as a convenient measure of material welfare, economic resources should yield equi-marginal net social products in all uses. This is an excellent example of an analytic proposition on which an analytic-hypothetical imperative might be based: it says, in effect, that if it is not advantageous to shift any of our resources out of their present uses into others, we are in an optimal position. It is by no means a useless formulation since by showing that to maximise the national dividend *is* to conform to this principle, Pigou gives us a valuable standard against which to assess existing economic arrangements or proposals. A statement like 'free competition will maximise economic welfare' is, however, synthetic and can serve as the basis only for a synthetic-hypothetical imperative. We might test it by trying to discover whether the spur of competition or the lure of monopoly profit leads to the higher output. Some economists have been guilty, perhaps unconsciously, of smuggling in all the propositions they wish to make in Welfare Economics under the umbrella of useful tautologies of the Pigovian type.

3 *Conclusion*

To conclude, can economists, despite their past mistakes and confusions, be of any help in the ordering of our practical affairs? We are justified in giving a cautiously optimistic answer. There are plenty of problems which the economist may not tackle very well, but is still better equipped to tackle than anyone else, if only because he recognises their existence more clearly.

> Was Marx right in supposing that capitalism has an inherent contradiction in it? What is the necessary minimum of governmental intervention into economic life? Can inflations and depressions be remedied? How far can the distribution of income be equalized without destroying the roots of economic progress? These and like questions cannot possibly be considered without serious study, and the name of this study is economics.[2]

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[1] Op. cit. Pt. II, ch. 3

[2] Boulding, op. cit. p. 239

What the study of economics demonstrably can do for us is to make us aware of certain inter-relationships: how complex and un-obvious these may be can be illustrated by considering how we try to work out the ultimate consequences of the imposition of a new tariff. That we can think coherently about these consequences must surely be accounted an achievement of economics. The economist has, too, a particularly valuable concept to contribute to any discussion of policy; that of economic, as opposed to technical, efficiency. The two coincide only by chance and it is often important that there should be someone to say so, loudly.

To make further progress with the subject, however, at least some of its theoretical assumptions will have to be re-thought; and re-thought in the light of further empirical observation, not of more work on the logic contained in our old postulates. We shall have to continue to work with theoretical entities whose definitions are, in part, a matter of convenience alone. But what the entities are, and how we define them, should depend much more than in the past upon what the real economic world is like. We should probably say less, even at the stage of elementary teaching, about perfect competition and more about actual market forms; and much less about the behaviour of the individual consumer and more about that of social aggregates.[1] Above all, we should cease to allow confusion over the logical status of our assertions to conceal the distinction between what we know about the economic world and what we have still to discover.

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[1] Cf. Little's analysis, op. cit., leading to the conclusion that ' the theory of price can begin quite legitimately with the demand curve '.

REVIEWS

DUHEM VERSUS GALILEO

The idea that a question has no meaning unless the truth of the answer to it can be decided by observation is current both among physicists and among philosophers. It is also customary to view this idea as novel and revolutionary, both in philosophy and in physics. The recent publications [1] of English translations of two great works by two people who excelled both in philosophy and in physics may at least help to dispel the myth concerning the novelty of this idea.

The idea that statements make no sense—or no 'empirical sense', as the ultra-modern terminology goes—unless they refer to observable facts suggests that the main function of science is to describe the observable world; that is to say, that it is the sole function of the scientist to organise known information and to predict future events. This idea is attributed to Scholasticism by both its adherent Duhem and its opponent Galileo. The Scholastics did believe in a metaphysical theory concerning the unobservable world, the hidden reality behind the appearances. But on these matters their views were fixed; and they interpreted the changes in astronomy since antiquity as bearing on appearances only, and not on the hidden reality behind them. A more modern trend, deriving from Berkeley and Hume, shares with Scholasticism the view that science must confine itself to appearances but denies the existence of a corresponding reality behind these appearances. The point of issue between Galileo and Duhem is whether science merely describes observable facts and is therefore simply an *instrument* for classification and prediction, or whether it should not attempt to describe the hidden reality—the *essence* of things, as it were.

The controversy between *essentialism* and *instrumentalism* (if I may follow Popper's terminology [2]) was topical in the time of Galileo, and again in that of Duhem. The declared problem discussed in the *Dialogue* is whether or not Copernicanism is true. But its main problem is whether or not the

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[1] *Dialogue on the Great World Systems.* Galileo Galilei, Salusbury's translation revised and annotated and with an Introduction by Giorgio de Santillana. Chicago University Press, 1953, pp. lviii + 506.

The Aim and Structure of Physical Theory. Pierre Duhem, trans. by Phillip P. Wiener, Foreword by Prince Louis de Broglie. Princeton University Press and Geoffrey Cumberlege, 1954, pp. xxii + 344, 48s. (The first edition was published in 1905, and the second edition in 1914.)

[2] See n. 1, p. 247

Copernican system is a meaningless mathematical instrument, useful for prediction, as Galileo's Scholastic opponents had it. The problem arose again in the time of Duhem, for reasons which I shall discuss below.

Galileo's declared aim in the *Dialogue* is to present all the arguments for and against Copernicanism. The value of these arguments is mainly historical. But the work contains much which is of a wider interest. I cannot discuss here the great artistic beauty of the book, the influences of ancient philosophers, and especially of Plato, the similarity to Descartes, and many other interesting points. But I shall discuss the major idea in the background of the *Dialogue*—the view that science should be separated from faith.

The story of Galileo is sometimes told with the tacit implication that Galileo was anti-clerical. In his interesting Introduction to his edition of the *Dialogue* George Santillana argues vehemently against this approach—an approach which misjudges the power of Galileo's religious feelings, and which amounts to saying that Galileo was a weak person broken by the Inquisition rather than an ardent Catholic who sincerely wished to co-operate with the Church as far as his conscience permitted him.

Santillana's presentation also brings out a problem which must still be considered as open. Why did the Church encourage Galileo to write the *Dialogue*, permit him to publish it, and then persecute him without being able to in fact produce any evidence that he had sinned ?

The usual answer to this problem is that the Pope was offended when it was suggested to him that Simplicius of the *Dialogue* was intended as a portrait of the Pope himself. This explanation (though not the identification of the Pope and Simplicius) is amply refuted by Santillana. The Pope had discussed the plan of the *Dialogue* with Galileo and the work had been given the *imprimatur* after it had been read by a whole array of chief censors. Moreover, the affair of the publication and of the persecution of Galileo was too important a matter to be motivated by the personal feelings of the Pope, who also was a friend and admirer of Galileo. In addition, Simplicius is a most mature and charming interlocutor. He takes a keen interest, he takes his defeats in good grace, and he is not offended by unfair remarks of his Copernican friends.

Why then was Galileo persecuted ?

My conjecture is that the Church wished to embrace Copernicus' system, in order to retain its intellectual hegemony ; that it wanted to make the transition from Aristotelianism as smooth as possible ; and that Galileo was supposed to help in this transition. This would explain why the Pope took so much interest in the preparation of the book. The long delay due to censorship shows that Galileo did not entirely satisfy the censors. His book satisfied their expectations in various ways, but he had been unwilling to compromise in questions of principle. Salviati, the author's mouth-piece, rebukes Sagredo, the enthusiastic layman, for committing himself to Copernicanism before the Pope had decided the matter. He also admonishes him to be more polite to Simplicius. Sagredo admits his error and recants. This incident recurs several times. The Church's attitude is presented as pro-Aristotelian but also as undogmatic. ' I do not doubt your rationality ', says Salviati to Simplicius. One of the censors admitted that he had read some of Salviati's statements with tears of joy ; which shows how near Galileo had come to establish a compromise. Yet Galileo had made it clear that the Church's authority in science was unacceptable, even though he himself had sincerely waited for the Pope to decide the issue in favour of Copernicanism before committing himself. Salviati criticises the acceptance of Aristotle's authority, and the existing method of reconciling his works with new discoveries, but does not attribute these faults to the Church, and he says almost nothing about the analogous approach to the Bible. He makes statements which are unfavourable to the Church in a very brief manner, and even repeats some of the official views of the Church, although in a non-committal way and often with irony, for which Simplicius criticises him in a friendly way.

In short, though I do not think that Galileo would have written the *Dialogue* as he did if he had been more free to speak his mind, I do think that throughout the *Dialogue* he meant what he said and that the constrictions he accepted were those he freely chose as a religious man. It seems to me that the Church was ready to grant more freedom to scientists provided it could save its face, and that Galileo, the ardent Catholic and leading scientist, was eager to help.

Why then did the plan misfire ? Because, I suggest, of the enthusiastic reception of the book : the Copernicans took the permission to publish the *Dialogue* as the Church's admission of failure. This is why the Pope decided to proceed against Galileo at once.

The First Day of the *Dialogue* is devoted to the criticism of the old system and the theory of truth behind it. Simplicius argues that truth is manifest,[1] both through observations and through common sense. Salviati contests this attitude. The senses do mislead us. Walking in the street we can see the moon following us like a cat on the roof-tops. Common sense does not object to inconsistent theories if the contradictions in them are not obvious and if we do not think very carefully. Attacking the theory that truth is obvious Salviati concludes the First Day by declaring himself a Socratic, and Sceptic.

On the Third Day the Copernican theory is propounded. Now Simplicius tries to show that nothing compels us to accept the Copernican system in spite of its by now admitted merits : truth is now hidden for him, beyond the reach of human reason and simple observations. Salviati attacks this attitude as dogmatic. ' In playing with you ', he tells Simplicius, ' a man shall never win but be always on the losing hand.' The method of accepting a theory merely because it does its job is uncritical and leads to stagnation. The fault with the Aristotelians is that they took seriously only that part or aspect of a theory which was known to have direct observational consequences. This allowed them to stick to most untenable theories. And it is also the reason why they never tested their theories. Tests are difficult to find, and only after we have actually deduced a testable statement from a theory can we be certain that this could be done. To look for no more than what we have already observed, in judging a theory, means in effect to exclude *new* tests. Moreover, the demand that the theory should merely agree with observation is too easily fulfilled. The Ptolemaic system satisfied this demand, yet Copernicus felt that truth must be simpler than the Ptolemaic system. Thus it was the fact that he regarded the theory as something which can be true or false, and not merely as a convenient instrument (a ' hypothesis ' as his opponents used the word, meant a ' mere instrument ', as was pointed out by Popper [1]) which led him to try a better idea. Furthermore, Simplicius' idea would be bound to arrest science, for it requires from any theory to accept what we ' see ', but what we see in its turn dependent upon our beliefs. We cannot start, and should not attempt to start with observation, but rather with reasoning.

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[1] For the discussion of the theory that truth is manifest see K. R. Popper, ' Die öffentliche Meinung im Lichte der Grundsätze des Liberalismus ', *Ordo*, 1956, 8.

Galileo insists that we must emphasise every problem in our theoretical system and argue about it until we can solve it. Galileo's only criticism of Copernicus is that he did not emphasise sufficiently a certain problem which he (and Galileo too) could not solve. If we wish to attain the truth we must break away from scholastic apologetics and stress, on the contrary, our difficulties. And only after all these difficulties have been overcome, and the utmost simplicity of our theory has been established, only then will truth be manifest.

Simplicity is the keynote of Copernicanism. Copernicus did not invent the heliocentric system but he succeeded in simplifying it. And although the simplicity of his calculations was understood only by the few, the intellectuals became enthusiastic about the heliocentric system because of the prospect of having a system simple enough to be comprehensible to all. Galileo tells us how surprised he was to find that the greatest scholars were those who wanted to shift the emphasis from scholarship to comprehensibility. He tells us that, when he was young, he did not bother to go to listen to a Copernican visiting lecturer because he considered him a crank or a madman, but that he was carried away by the enthusiasm of those who went to the lecture. The enthusiasm was shared by people who knew very little about astronomy but who now saw some hope that they would be able to understand it.

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[1] See K. R. Popper, ' A Note on Berkeley as a Precursor of Mach ', this *Journal*, 1953, 4, 26 sqq.

Galileo offers alternative theories, and his views are tentative. But he argues from a position of strength. He is convinced that truth is simple, and that we can create simple geometrical models of the universe and argue about them until we have found the truth.

I have tried to focus upon those aspects of Galileo's philosophy of science which show most sharply the contrast between his views and those of Pierre Duhem (1861–1916). For Duhem was not only one of the most distinguished instrumentalist philosophers of all time—he also was the only instrumentalist philosopher who in fact answered Galileo's challenge. Instrumentalists have argued very little against essentialism, and none of them except Duhem has attempted to answer Galileo's criticism. Duhem's answer to Galileo is that not instrumentalism, but essentialism, is the view of science which leads to obscurantism.

The history of science was written almost entirely by essentialists. This is one of the reasons why medieval science was almost totally neglected. Duhem undertook vast and most interesting researches in pre-Copernican physics. He tried hard to show that there was continuity of progress towards Copernicanism, attributing, for example, the invention of the law of inertia to a scholastic of the thirteenth century. (By the way, Galileo did not claim priority for the law which he puts in Simplicius' mouth ; Salviati induces him to admit that it follows from some of his own assertions, and to agree that this invalidates an important objection to Copernicanism.) Duhem's thesis is that if physics is viewed as an instrument, history becomes coherent and organic—a history of ideas constantly developed and improved by people who often were conscious instrumentalists, and who always acted according to the instrumentalistic theory. But if physics is viewed as the discovery of the essence of the physical world, it becomes dependent on metaphysics with the consequence that the continuity of its history breaks down, and is replaced by a chronology of arbitrary and conflicting metaphysical dogmas.

Duhem wrote at a time when the field theory had succeeded in shaking the confidence of some leading Newtonians who started (in the manner of Simplicius) to argue that they did not really or literally mean what they said. Duhem was impressed by the fact that Helmholtz accepted the field theory while insisting that it is a mere mathematical fiction and that the field does not really exist ; and he was also impressed by Poincaré's insistence that the Newtonian principles are implicit definitions and cannot tell us anything about the world. He saw in all clarity that this was a break with the traditional rationalistic approach, and he hoped that the disturbing conflict between science and the Church (which he felt was a mere misunderstanding) was now ripe for a negotiated settlement.

Duhem made no secret of the fact that his passionate interest in the problem was connected with his religious views. (He was an orthodox Roman Catholic.) But he was deeply convinced that his arguments were in no way biased. Yet his approach was very different from that of Poincaré or Mach, whom he was proud to view as his allies. For unlike Mach he was not much interested in the difficulties inherent in Newton's theory of gravity and unlike Poincaré he never saw the point of Maxwell's theory. On the contrary, among the three he was the most ardent Newtonian. Although he argued that Newton's theory is fictitious—a mere instrument—and although he contested Poincaré's assertion that it is preferable to stick to Newtonianism under any circumstances, he stuck to his Newtonian views until the end. He derided the enthusiasm for Einstein as a ' frantic and hectic race in pursuit of a novel idea [which] has upset the whole domain of physical theories, and has turned it into a real chaos where logic loses its way and common sense runs away frightened '. (He did not even bother to name the ' novel idea ' ; but this attack, placed in the preface to the second edition of the present work, which appeared in 1914, is quite unambiguous.) Indeed the ' novel idea ' did ' upset the whole domain of physical theories ', but why should a progressivist object ?

The answer may be found in the first part of the present volume which was first published as an independent essay. It consists of four chapters. The first two chapters present the two views on the aim of science, which I label here (following Popper) as ' essentialism ' and ' instrumentalism '. The third chapter is about the history of the views. Here we learn that Kepler was naïve and that Descartes was proud and confident. As to Galileo we are only told that his judges were right in deciding that he only pretended to be an instrumentalist, and that they were right in their philosophical judgment, too. (This is far fetched : they did not bother about philosophy.) The fourth chapter is psychological : it classifies mind into two categories, the concrete and the abstract, or the English and the French. Here he pours out all his hatred of English inductivism and scientific realism. He is as unfair as he is cheap. He ridicules the English method of writing vectorial formulae (which was later improved by Continental scientists and is now universal) and he attributes Green's discoveries to Cauchy. He attributes the idea of abstract electrostatic fields to all the continental physicists and he alleges that it is a notorious fact that, by contrast, Faraday invented a mechanical model of electrostatic action (regrettably he gives no reference to this alleged ' fact '). Inductivism being essentially English, he considers that the Continental inductivism can only be a ' diffusion of the English methods '. The craze for a mechanical model is also typically English ; and while Galileo's and Descartes' interest in mechanical models are explained as a reaction to scholastism, he has no excuse for these unimaginative Englishmen, Kelvin and Maxwell (of course,

' English ' in the French sense even more than in English-speaking countries often means Scottish).

Duhem does not make explicit the point of this farrago—but it does have a point, and a very interesting one too. Duhem identifies ' abstract ' with ' imaginative ' and therefore with ' fictitious '. It follows, then, that ' real ' is identical with ' concrete '. Hence, as Faraday insisted that the electromagnetic field is real it follows that he must have thought of the field as something concrete like a piece of rubber. Realism is a psychologically explicable error, similar to sense illusion : it is an attempt to deceive oneself that the abstract is concrete. That is why unimaginative people are attracted to realism. And that is why realists believe in mechanism. But this realism is dangerously dogmatic. While there can be no room for a display of the imagination in matters of concrete facts, our theories can and should be abstract, bold, and imaginative.

Let it be quite clear that this attempt of mine to connect the fourth chapter with the previous three is not unobjectionable. It is difficult to reconcile Duhem's statement that scientific theories ' enjoy universal consent ' (p. 10) with his Galilean appeal for freely imagined abstract theories. But the demand that we should imagine theories freely is explicitly made in the second part of the work, and the argument that scientific theories ' enjoy universal consent ' cannot be taken seriously anyhow, not so much because it is, historically, palpably false, as because, within Duhem's own philosophy it does not signify : we neither can assent to nor dissent from a string of words which has no independent meaning.

The great controversy over Einstein's theory was raging during the interval between the two original editions of the present work. It did not at all fit into the way in which Duhem was used to seeing things. If he was not prepared to change his view, he could only ignore all this as yet another little incident of minor importance. ' They ', Duhem tells us scornfully, speaking of ' certain schools ', ' they think they can run all the more easily and quickly from one discovery to another . . . ' while in fact they evict logic and common sense.

It is a bit difficult to understand these accusations. As to common sense, Duhem's leading idea was that all theories, save that of Aristotle's, are abstract, fictitious, and counter to common sense. From which he should have concluded that if common sense and science never went together, then ' certain schools ' could not evict common sense. As to logic, it does not seem to me that Duhem had any right to speak in its name in this context. An instrumentalist has no reason to demand that his various ' fictitious ' theories cohere : they are all instruments, and logical coherence between them is unnecessary, since they do not aspire to be true. For example, the fact that Newton's theory did not cohere with electrodynamics, did not prevent Duhem from accepting them both. (Yet he condemned

Maxwell for having two alternative theories of the dielectric ; which is a strange, but not necessarily an unfair condemnation if Duhem had in mind that Maxwell was not an instrumentalist but an English realist and thus had no excuse for this incoherence.)

In fact Duhem said quite clearly that an instrumentalist has no need to be coherent in this way ; and if he nevertheless hoped for coherence, he admitted (p. 220) that his only ground for this hope was ' an intuition we are powerless to justify.'

Now I believe that it is this intuition which is essential to the interest and fascination of Duhem, and especially of his history of science. It alone makes this history coherent, and progressive. The admission that he could not justify his intuitive demand of universal consistency therefore amounts to a declaration of bankruptcy as far as his analysis of science is concerned.

Duhem's intuitive demand for universal coherence between theories is of course equivalent to a demand for realism. And he knows this, for he hopes and believes, that science will approach something like a *natural classification* of the real essence—in fact to Aristotle's physics (Appendix I, 9). (De Broglie also comments on the strangeness of this retreat to realism in his exciting preface to the book, written from a realistic viewpoint.)

Duhem, however, proposed very strong arguments in favour of his thesis that if we are really sceptical and open-minded we must accept his instrumentalism. He tried to show that there can be only two views of science. And he tried to show that he who does not accept his view must accept the theory that science has acquired the happy state of near perfection, that we have achieved the truth, if not totally then at least essentially. If we are realists, we must be essentialists, we must believe that we have found the essence of reality. And thus, he concluded, we must become dogmatists.

Duhem's argument may be presented in this way. We must admit that we do not know everything, that there is room for improvement. The way to show that our theories are not yet perfect is to refute them, to find a fact which is contrary to a prediction based on them. This means that our theories are likely to be false unless we possess the ultimate truth. One cannot even say that a part of our theories, say, mechanics, is absolutely true while other parts, say, electrodynamics, are tentative. The distinction between these two branches is ours, and we cannot even fully mark it, because we know too little of electrodynamics. The distinction between the two branches is a part of *our* theory which is open to refutations (and which indeed was refuted by Einstein). If we take the history of physics between the fifteenth and the nineteenth centuries, we see clearly that there was great progress, though strictly speaking the nineteenth century's theories are false. This progress, then, cannot have been towards truth. The realist thinker is tempted to say that it was progress towards truth because the later theories, even though false, are ' essentially ' true. Thus, Duhem explains (and, I think, very convincingly) why realists tend to fall back on essentialism. But, Duhem argues, to say that we are ready to learn, to accept criticism, while insisting that we are essentially right, is both naïve and hypocritical ; in addition, it is the fallacy of mixing physics with metaphysics (to use Bacon's idiom). I think it would be preferable to argue that this is a bad and obscurantist metaphysics. But Duhem could not say this since in metaphysics he is essentialist too: he insists that the Aristotelian system of ' natural classification ' is essentially right in spite of all the mishaps and refutations which it had to suffer.

Let us, however, leave Duhem's metaphysics and return to his criticism of scientific dogmatism. He gives in the last chapter of the present work a specimen of his vast historical research—and it is a superb piece of research —to show that physics has progressed gradually, by small changes, now in this respect, now in that, so that it is absurd to declare that at a certain stage physics got hold of the essence of truth and since then has only eliminated minor errors. If we forget essentialism then we are faced either with the problem of how physics approaches truth by transitions from one false theory to another, or with the admittance that the immediate aim of physics, the one we observe to be partially attainable, is not truth (as he suggests, unless he remembers his ' natural classification ') but utility.

This is Duhem's strongest argument. We cannot attain truth by a series of false theories. Truth remains the *unattainable* ideal, the regulative idea (to use Kant's idiom). Therefore in each stage our theories are, most likely, false. (They may include, perhaps, some true statements, but since they are tested as wholes, they are, in all likelihood, false as wholes.) In what sense, then, can it be said that we did progress, if our theories are false ?

In the second part of the present work Duhem offers as a solution a new theory of truth, or rather, a new theory of meaning. A physical theory is *true*, according to Duhem, if it agrees with all *known* facts, all *observed* facts. It has no other *meaning* than the total meaning of all the *known* statements of fact which can be deduced from it and which were verified by observation. We can of course deduce new statements of fact from it and thus test them ; but then the verification will change, and enrich, the meaning of the theory. It may of course be said that if the result of the test is negative the theory is refuted. But this ' refutation ' would not be one in the sense of the realist, but a ' refutation ' in a purely pragmatic sense of finding a limit to its applicability. Now we can see how science may progress from a useful to a still more useful theory. *There are no degrees of truth in the ordinary sense of the word, but there are degrees of pragmatic ' truth '—of usefulness.*

Duhem's theory of meaning (or rather of empirical meaning) is, it seems to me, the best of its kind. Duhem showed that it is preferable to Poincaré's theory of meaning. For Poincaré scientific assertions are everyday assertions of observed facts put in a compact manner. 'Here runs a current' means for him, for example, the statement of brute fact like 'The needle of the galvanometer moves'. Duhem argued that it *also* means 'Gas bubbles appear in the battery' and *also* 'Whenever the needle of the galvanometer moves there appear gas bubbles in the battery'. Poincaré's reply was that the language of scientific theories is such that these observed effects are all said in one sentence—that science is a sort of a shorthand. To this Duhem replied that, if knowing the meaning of a theory meant knowing all the verifications of its consequences, then we could not test it any further.

Duhem's theory of empirical meaning is not, however, his whole theory of meaning. It is only the theory of 'meaning in physics'. There can be also a mathematical meaning (here Duhem refers to Hadamard) and a metaphysical meaning. This enables him to say that the same statement may be meaningless in physics and meaningful in metaphysics. He tried to show that this is necessarily so. Yet even if it were necessary and convincingly so, to me it would remain a horrible idea. It deprives science of its value (except its pragmatic value which does not interest me overmuch); it thereby vulgarises science, and it makes metaphysics a kind of religion—or part of religion.

Fortunately the idea is most unconvincing. (1) From the pragmatist point of view, we are not interested in logic or in history or in hairsplitting arguments. (2) From the epistemological point of view Duhem cannot explain how some purely fictitious ideas enable us to predict future events. Indeed he admits that they have a kernel of theoretical truth in them. But to the extent to which he admits this, he is a realist, and implies that scientific ideas are not purely fictitious. (3) From the point of view of historiography Duhem's interpretation is defective. It entirely ignores the terrific impact that purely metaphysical doctrines had on the history of science, the great inspiration which science drew from purely metaphysical doctrines like the indestructibility of matter, the soul of nature, or atomism. (The adverse effect of Duhem the epistemologist upon Duhem the historian may be illustrated by his derision of Gassendi: here he is no longer a historian but a partisan.) (4) It ignores the impact of science on metaphysics (whose importance might be shown by the history of determinism). (5) It detaches science from humanism and endangers the tradition we inherited from the Renaissance.

However, Duhem's arguments against scientific realism are too strong to be ignored or to be left unanswered. He is very convincing both in his attack on essentialism, and in his argument that the choice is between essentialism and instrumentalism. Indeed we have only to remember that Galileo, in spite of his professed scepticism, fell back on essentialism when arguing against the philosophy of Duhem's predecessors. But somewhere there lies an error. Both Galileo and Duhem (when he remembers his 'natural classification') would agree with what has become a commonplace since Einstein—that *we approach the truth by a series of approximations.* This is the genuine form of the sceptical attitude which Duhem wishes to adopt since it admits that there is always room for improvement. Yet neither of these two theories accounts satisfactorily for this situation. The essentialist view asserts that truth, if not at hand, is round the corner. The pragmatist view asserts that the progress is purely technical, not intellectual. We must therefore find a third view which will explain more satisfactorily how we can progress while holding only tentative conjectures which we hope will be superseded. And for the sake of doing so we must find the error in the argument, common to Galileo and Duhem, in favour of the assertion that there can be only two views concerning human knowledge, or science. The starting point must be, I believe, that Galileo's scientific realism is the better of the two views. Galileo's arguments against instrumentalism were never answered ; this should be borne in mind even though we have to admit that his essentialist theory of scientific realism is unsatisfactory. He made a brave attempt, and if he failed he was still right, 'essentially', and in what matters most : his case for intellectual independence needs no advocacy.

Perhaps I might say at this stage that my criticism of the 'two views' is an adaptation of Popper's criticism in his 'Three Views Concerning Human Knowledge',[1] and that his third view seems to me to answer the question without falling into the same difficulties as the previous traditional views. But let me first re-formulate the problem again—the *problem of knowledge.* I wish to pose it in a form which, though alluding to Kant, is new, I think : *how is partial theoretical knowledge possible ?*

Popper's simple answer to this question is that we learn something about the truth (although not the whole truth itself) by learning from our mistakes. In this way we can make discoveries. In his *Logic of Scientific Discovery* Popper describes theories as questions put to nature. And even though we may argue that our fundamental questions about the nature of the physical world and about man's place in it are never answered, it cannot be denied that we can pose more and more intelligent questions. The more intelligent the question the more we can learn from negative replies to it. The statement that Newton's theory is false is both true and *theoretically more informative* than the statement that Kepler's theory is false. There is no room for deriding science because its theories are actually refuted or likely to be false. The only rational manner of discussion is to try to refute the present theory by concrete argument. By a concrete empirical argument, by a refutation, we transcend the limits of our knowledge and open new horizons for a better theory, for a better formulated question. When

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[1] *Contemporary British Philosophy*, 3, ed. H. D. Lewis, London, 1956. The idea of a third view occurs already in Popper's 'Note on Berkeley', see n. 1, p. 240.

Simplicius argues vehemently against Copernicanism, Salviati encourages him and asserts that he does not doubt his rationality, but when Simplicius accepts the Copernican system as useful though probably false Salviati reproaches him for being an irrationalist. I believe that Duhem's fault was not that he opposed Maxwell and Einstein, but that he condemned them on general grounds instead of arguing to the point.

But Popper's philosophy of critical discussion need not be confined to physics. It distinguishes between physics and metaphysics by characterising physics as a discussion which involves experimental arguments (to use Faraday's idiom). It thus explains the development of metaphysical doctrines into scientific ones, and the fact that science does influence our metaphysical beliefs if we do not create an iron curtain between the two. Galileo's chief thesis was that the new science had to reshape our metaphysical beliefs. He argued that the acceptance of Copernicanism *together with an attitude of searching for a more and more comprehensive picture of the world* was revolutionary—more revolutionary than the acceptance of Copernicus' theory. Galileo tried to show what the Copernican way of thinking could mean if we re-interpret all the known facts with its help. He tells us (in the Fourth Day) that his attempt to have a theory of the tides which will fit his picture of the world nearly drove him mad. We may smile [1] at Galileo's attempt to explain everything by a law of inertia. But even though his idea that such assumptions must be essential and ultimate was uncritical, his brave attempt to take them as ultimate and to argue for them scientifically was not only fruitful, but necessary : someone had to make the attempt to find out how far we could proceed with a minimum of assumptions. (To my knowledge, it had not been seriously attempted before with Democritus and Plato.) Science cannot proceed without a metaphysical hankering after the secret of the world, even though it is quite naïve to believe that the interest is satiable. As Einstein put it : [2] ' There exists a passion for comprehension, just as there exists a passion for music . . . Without this passion there could be neither mathematics nor natural science. Time and again the passion for understanding had led to the illusion that man is able to comprehend the world . . . by metaphysics. I believe that every true theorist is a kind of a tame metaphysicist . . . '

J. AGASSI

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[1] Santillana subscribes to the description of Galileo's theory of the tides as ' Galileo's folly '. This seems to me too harsh. His ultimate model of the tides was very ingenious, and its refutation encouraged the return to dynamical theories of celestial action. Thus his failure is not less important than his success.

[2] ' On the Generalized Theory of Gravitation ', *Scientific American*, 1950, **182**, reprinted in his *Ideas and Opinions*, 1954.

PAVLOV RECONDITIONED

THIS book[1] consists of a representative selection of Pavlov's writings, from his earliest papers to his last recorded discussions. It includes little from his books, which are readily available, but much from sources that are otherwise not easily accessible. Seventy pages at the end are devoted to verbatim reports of the discussions that took place every Wednesday at his laboratories in the years 1934 and 1935 ; here he presents his general views and opinions rather than experimental details.

The translation is usually adequate, though sometimes clumsy. Printing and paper are not of the best, and the fount shows a curious tendency to change from line to line for no obvious reason. There is no index.

The introduction is written by K. S. Koshtoyants, but there is nothing to show whether he made the selection or wrote the thirty pages of notes and comments. The internal evidence, however, suggests that he did ; for, throughout the book, every comment by the editor shows the same aims—to build up a picture of Pavlov as a great genius who was always good and right, and to show that Pavlov always fought valiantly for materialism against idealism. Fortunately, the editor's preoccupation with Communist orthodoxy is easily ignored, and through the haze of Red incense a clear picture can nonetheless be perceived.

Since some of the material is new, the time seems ripe for a re-appraisal of Pavlov's personal status as a scientist, and of the status of his work in the impersonal world of science. In the late twenties it seemed so clear —Pavlov was a genius and the problem of the cerebral cortex in action was practically solved. Thirty years later, however, we are not so sure We notice with surprise that the intervening thirty years have brought no advances of major degree, only an addition of minutiae to what was already known. There is no reason to suppose that the Russians have made any significant advances, in spite of the fact that at one time they had hundreds of workers engaged in the project. On the contrary, the fact that today's physiologists in Russia spend their time arguing the exact interpretation of the old Pavlovian scriptures suggests that they are stagnating, and merely concerned with whom to blame for the failure. Projects that are advancing successfully do not require scapegoats, neither do they trouble to appeal to the authority of the past—they let today's achievements speak for themselves. It seems clear, then, that the science of conditioned reflexes has become bogged down ; why ?

Not only has the science not come up to expectations, the practice also has been disappointing. We still do not know whether the conditioned reflex is or is not important in everyday life. Pavlov had no doubts in the matter, but Hull, who spent a lifetime trying to use the concept of conditioning in psychology, achieved nothing but a monument to the thoroughness of his attempt. Today the concept lives vigorously in the books about psychology, but does it live outside the books ? In medicine, that most practical of all sciences, it is used not at all. It is true that some methods of treatment (e.g. of giving the chronic drunkard alcohol and an emetic together) use conditioning ; but such simple methods of training were known long before Pavlov. The more specifically Pavlovian discoveries, those of irradiation, of induction, of ' paradoxical ' phases, etc. —are, so far as I am aware, used by nobody. They are talked about, invoked as explanations, but not used as working tools, not used as trustworthy guides to action. Evidently they don't work out of Pavlov's hands.

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[1] I. P. Pavlov, *Experimental Psychology, and other essays*, Philosophical Library, New York, 1957. Pp. 653. $7.50

By this remark I do not for a moment wish to question Pavlov's honesty. Everything we know of him, the internal evidence in his papers, and the testimony of those who confirmed his results, suggests that he was basically quite honest. In the penultimate chapter of his book, *Conditioned Reflexes*, for instance, he lays before the reader new experiments that practically make nonsense of the simple explanations that he has given in the earlier chapters, explanations that sounded, in the earlier chapters, so perfectly plausible. Thus, throughout the discussion of extinction by non-reinforcement he takes for granted that non-reinforcement will explain, for obvious reasons, the lessening of the salivation. In the penultimate chapter he announces the perplexing fact that while conditioning develops rapidly under full reinforcement, it develops even faster if the alternate presentations are *not* reinforced ! Clearly the naïve ideas used initially are seriously in error. But Pavlov announces the fact, and thereby shows that we need not suspect his honesty as a reporter. What is suspect is whether the whole work is not what Bernard Shaw said all science is—a put-up job ! Let me explain.

Pavlov grew up when ' psychology ' meant either associationism—the belief that all mental and behavioural effects could be traced to simultaneous, or just prior, physical events demonstrable in the real material world—or meant the attribution of mental and behavioural effects to non-observable, non-material causes such as volitions, wishes, entelechies, etc. Pavlov chose the former and accepted it totally. At the age of eighty-five he could say ' . . . the same can be said of our thinking : beyond association there is nothing more in it ' (p. 557). The experimental dog was thus in the hands of an extremely skilled and patient experimenter who knew exactly what he wanted. Though the difficulties were great, triumph came at last, and the dog showed—association ! Was Pavlov a real discoverer, or was he like the sculptor who demonstrated that the statue was ' really ' present in the block of marble all the time, certain adventitious matter having to be removed simply because it was obstructing the view! The answer at present must be considered open to varying opinions. The reviewer cannot suppress the feeling that had the century produced ten experimenters of such skill, each taking for granted that the brain worked in some one of ten different ways, we would by now have had ten different theories, each supported by its own irrefutable demonstrations that the brain works in just that way. The truth is that the dog's brain is amply flexible enough to demonstrate ten different ways of working, provided it is given ten environment-experimenters each systematic and constant enough in its own characteristic pattern. In this sense, then, the conditioned reflex may well be, as Deutsch puts it, ' an experimenter's fiddle '.

With regard to the more complex experiments performed and phenomena demonstrated, there can be little doubt that Pavlov has added permanently to the world's knowledge of what happens when the dog's brain is given various complex patterns of stimuli and reinforcements. And some of this knowledge is really new and surprising. An experiment such as that described in *Conditioned Reflexes* (p. 197), in which the dog remained quite unable to secrete a drop of saliva though acid was injected repeatedly into the mouth remains a tribute to Pavlov's skill as an experimenter and a challenge, a new Rosetta stone, to all who would deduce its meaning. Pavlov provided the facts; he was quite unable to make the deductions, nor in fact to use any but the most elementary and naïve concepts. That he was no theoretician is shown most clearly in the last section of this book. Problem after problem is ' solved ' by a direct recourse to the simple and obvious. Thus Sherrington's *The Brain and its mechanism* coming somewhat into conflict with his views, he resolves all difficulties thus: ' I simply suppose that he is ill, although he is only seventy years old,—that these are distinct symptoms of old age, of senility ' (p. 565). And that was the end of the matter, so far as he was concerned.

The position today, then, seems to be that Pavlov educed a wonderful set of facts from the dog's brain, but he could do nothing towards their interpretation, which is still awaiting the Sherlock Holmes who can show us how obvious it all is! His own interpretations (in the concepts of the muscle-nerve preparation and of connection) were so inadequate that the beginner is probably better advised to avoid them like the plague, till the beginner's own wit and talents have got him well started on the right lines. Even when he reads Pavlov's descriptions he must be alert to distinguish what Pavlov did from what Pavlov thought he did; one example will suffice. In certain experiments Pavlov says that the stimulus (bell, say) was always reinforced. He also says that the bell was given for some seconds before the meat powder was given. Thus, *in fact*, for several seconds the dog received the compound stimulus, ' bell, but no food ';

the fact that *Pavlov* knew that the food was going to be given later is quite insufficient to make this 'reinforcement' so far as the dog is concerned.

Up to the present, at least a dozen mechanisms of various types (neuronic, electrical, stochastic, etc.) have been shown to be capable of giving elementary conditioning, but none has been able to explain the more complex patterns of response that Pavlov so skilfully displayed. Perhaps the new logic of mechanism as described in my *Introduction to Cybernetics*,[1] may enable the problem to be solved and something of the basic mechanisms to be deduced. The possibility seems promising. Pavlov states explicitly (p. 422) that by 'reflex' he means those of an organism's activities that can be related to the physically observable, material, conditions in its surrounding that preceded the activity. If we allow the activity also to depend on an inner physiological state (that is also demonstrably consequent on physically observable surrounding conditions), then Pavlov is manifestly trying to treat the organism as a 'machine with input'; and all the logical methods now applicable to this form should be applicable to the various protocols given by Pavlov in his publications. This task has yet to be done, but its execution cannot be long delayed. Whoever undertakes it should, in the reviewer's opinion, make the greatest possible use of Pavlov's facts, while refraining in every possible way from paying the least attention to Pavlov's opinions! Only then will Pavlov's greatness be appreciated without qualification.

W. Ross Ashby

The Organisation of Science in England. By D. S. L. Cardwell.
William Heinemann, 1957. Pp. x + 204, 18s.

This book describes the development of the practice of science especially in so far as it is reflected in scientific education in England from the end of the eighteenth century to the end of the First World War. It forms a useful reference book for those interested in this field of education. Some of the arguments for and against what should be taught, for the expenditure of money, on the importance of technology, and so forth are as applicable today as they were in the days when they were advanced.

From the point of view of the philosopher of science perhaps the most important discovery in the book is the attitude of the learned at various times to the inclusion of some philosophical training in scientific syllabuses. When London University instituted its degrees in science in 1858 logic was part of the syllabus. By 1900 this had disappeared in favour of increased specialisation. Further back still, in 1831, the Oxford Public Examiners, in discussing the syllabus for an examination in mathematics (the first subject to have a special examination devoted to it), submitted that 'logic be not absolutely required of candidates for Mathematical Honours' because 'the requisition put on candidates for Mathematical Honours to pass in logic acts as a direct discouragement to Mathematical studies, besides being in itself unjust, as the candidate has not the option allowed to others in the general examination, but is obliged to undergo an examination in logic, in addition to mathematics'.

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[1] W. Ross Ashby, *An Introduction to Cybernetics* London, 1956

Perhaps it was this attitude to which De Morgan referred when in 1839 in a note on the flyleaf of his 'First Notions in Logic: Preparatory to the Study of Geometry' he said that logic is 'a science the study of which would show many of its opponents that the light esteem in which they held it arises from those habits of inference which thrive best in its absence'. By 1847 Boole had published his first book showing that it was possible to formalise logic, thus giving it not only a mathematical appearance but also some mathematical power. Although the influence of the logicians on the mathematicians was declining in 1879 Frege showed that the foundations of mathematics could be discussed as a branch of logic. By this time several generations of mathematicians had arisen who could have been unaware of the possibility of a connection between the two disciplines. The Oxford Examiners had not realised the importance of the study of basic concepts in their subject. Yet the modern schools of analytic philosophy can be traced to the Boole-Frege movement.

Unfortunately, if we peruse the requirements for many scientific degrees in this country today we are still struck by the lack of awareness of the importance of training scientists to examine their basic concepts. It seems anomalous that those who practise science as a profession should remain virtually untrained in logic and scientific method. This is, as Dr Cardwell's book shows, the price of specialisation.

R. F. J. Withers

Foundations of Biology. By Felix Mainx. (*International Encyclopedia of Unified Science*. Vol. I, No. 9.)
The University of Chicago Press, Chicago, 1955. Pp. ii + 86. 15s.

This is one of the few numbers of the Encyclopedia to deal specifically with problems of biology. Professor Mainx has rendered a signal service by his contribution and one is no less grateful to Professor Woodger for his translation.

Some restrictions are necessary in a volume such as this, but there is a valuable section discussing the basic disciplines used in biology, morphology, physiology and genetics, as well as an account of the more complex interactions between these methods of approach. The final section is an essay on the value of speculation in biology. In these two sections which, apart from a brief introduction comprise the work, much valuable material is discussed in a stimulating manner. This discussion is the more valuable because the writings of Hartmann, Bünning, and Ungerer are insufficiently known in this country. However, one might have expected to find in this contribution to the *Encyclopedia of Unified Science* some mention of N. Rashevsky's views on the applications of topology to problems of biology.

In the last section Professor Mainx rightly maintains that a 'philosophy of life' (*Weltanschauung*) is not to be derived from empirical science. This is true, largely because of the dependence and imperfections of a human mind. Because of their reliance upon sense-knowledge and observations, and the imperfections implied thereby, our minds are limited and given what is perhaps an unfair bias towards empiricism and because of these limitations thus imposed upon our knowledge, a complete 'philosophy of life' is not derivable from empirical science. It is perhaps unfortunate that one receives the impression that Professor Mainx regards this conclusion more as a personal conviction than as a statement based on reason. There is also some difficulty over the author's seeming distrust of 'final causality' and of 'anthropomorphic psychologizing' (p. 73). Now, in one sense, any human mental activity may be called anthropomorphic psychologising because a theory is an act of the human intellect and based upon the human senses. I imagine that Professor Mainx would concede that the human intellect owes some dependence to that which creates both it itself and the subject matter of its speculations. It would thus be not unreasonable to find traces of this creation both in our minds and in their objects, however differently such traces may appear. Thus there seems no fundamental reason to object to the explanation of a natural phenomenon in 'anthropomorphic' terms, provided that these be taken analogically.

This view would seem to be consistent with the beliefs of the author upon which, one gathers, his 'philosophy of life' depends.

D. F. Cole

William Heytesbury: Medieval Logic and the Rise of Mathematical Physics.
By Curtis Wilson.
The University of Wisconsin Press, 1956. Pp. xii + 219. $4.00

This book, printed in facsimile of typescript, is the third of a series of Publications in Medieval Science, and it is a study of Heytesbury's *Regule Solvendi Sophismata*. Heytesbury, who became a fellow of Merton College in 1330, was one of a group of fourteenth-century Merton logicians whose works attained European celebrity and were still in such demand at the end of the fifteenth century that enterprising Venetian printers found it profitable to print them together with the copious commentaries of the scholars who had lectured on them at Italian universities. It may at first sight seem odd that anyone should try to connect *sophismata* with mathematical physics, and Mr Wilson has to admit in the end (p. 149) that ' with the exception of the conclusions regarding distance traversed in uniformly accelerated motion, which were common knowledge in late medieval times, it is not clear that the results set forth in the *Regule* played a rôle in the formation of the empirical-mathematical science of the seventeenth century '. But it is by no means fanciful to suppose that there might be a connection; for Heytesbury's last three chapters, which Mr Wilson considers in detail, are concerned with puzzles about continuity suggested by Aristotle's writings on physics, and they show ingenuity of the same sort as was required in later times for dealing with the philosophical difficulties of mathematical analysis. In particular the chapters *De incipit et desinit* and *De maximo et minimo* elaborate Aristotle's distinction between an intrinsic and an extrinsic boundary, first with respect to the durations of temporal processes such as living and next with respect to the ranges of powers such as that of lifting weights, while the chapter *De tribus predicamentis* discusses the notions of velocity and acceleration with reference to the three different types of change which Aristotle recognised, namely, local motion, growth, and alteration.

To our way of thinking the discussions are very curious because they are neither clearly mathematical nor clearly physical in their general conception. Thus it was asked, for example, whether there was a maximum weight which Socrates could lift or a minimum weight which he could not lift. Since it is obviously impossible that such a question should ever be settled by experiment, we are inclined to dismiss it as meaningless, but medieval logicians debated it much as pure mathematicians now discuss questions about the boundaries of sets of real numbers. The arguments they produced were often based on unsatisfactory assumptions, but they were very acute and it is clear that they derived great satisfaction from the subtlety of their own distinctions. One of the surviving discussions *de maximo et minimo* mentioned by Mr Wilson (p. 101) forms part of a commentary on the theological *Sentences* of Peter Lombard. The author, Roger Rosetus, asks whether a *frater* could be obligated by the precept of a prelate to the performance of a task such as the reading of holy scripture even if this were against his conscience, and he considers as a possible objection the rather far-fetched argument that there is neither a maximum act of studying which would conform to the injunctions of the superior nor yet a minimum act which would not conform, since the intensity of study may be varied continuously *in infinitum*. Having introduced the topic in this curious fashion, he then proceeds to an analysis of the conditions in which maxima and minima are assignable. Was all this ingenuity wasted? It would be pleasant to think that it did something to prepare the way for modern science, and we know at least (p. 112) that Galileo studied such questions in his youth; for there exists among his juvenilia a discourse on maxima and minima which is probably a transcript of lectures on the subject given at Pisa in his time or shortly before.

Mr Wilson's work is almost entirely exposition of texts. For each topic he sets out first the passages of Aristotle from which the discussions started and then the views of Heytesbury and other medieval writers who dealt with it. In his anxiety to report his sources faithfully he sometimes produces English paraphrases which read very queerly (e.g. ' Any intensive degree of a latitude contains in it all the more remiss degrees of the latitude ' (p. 140)), but it is difficult to see how an expositor could avoid medieval terminology without risking misunderstanding, and it must be said in justice to Mr Wilson that he gives careful explanations of the technical terms which he retains in English form. In his prefatory note he says that he learnt from Professor Lynn Thorndike of Columbia University ' what is meant by patience and exactitude in historical research ', and there can be no doubt that by his labours on manuscripts and incunabula he has assembled here a great deal of information which was not previously accessible. If the doctrine of the fund of merit applied to university examinations for the D.Phil. degree, he might even be allowed to help some less industrious brother into the heaven of scholarship.

On page 7 it is said that Heytesbury became ' *bursar* (i.e. recipient of a scholarship) ' at Merton in 1338. But surely the bursarship to which he was appointed eight years after his election to a fellowship was the treasurership of the college? On page 34 the phrase ' *res permanentia* ' occurs several times as though it were the plural of ' *res permanens* '.

William Kneale

International Encyclopedia of Unified Science. Vol. I, Parts 1 and 2.
Edited by Otto Neurath, Rudolph Carnap, and Charles W. Morris.
The University of Chicago Press. 41s. 6d. each.

These two volumes are an important reprint of ten numbers of the *International Encyclopedia of Unified Science* which began to appear in 1938. The contents are so diverse that it would be impossible to discuss them all in a short compass. What the volumes contain are the following. Various aspects of Unified Science are discussed by Neurath, Bohr, Dewey, Russell, and Morris. The Theory of Signs is discussed by Morris ; Logic and Mathematics by Carnap ; Linguistic Aspects of Science by Bloomfield ; Procedures of Empirical Science by Lenzen ; Probability by Nagel ; Physics by Frank ;

Cosmology by Finlay-Freundlich ; Biology by Mainx ; and the Conceptual Framework of Psychology by Brunswik. It will be seen that these are contributions by well known exponents that philosophers of science cannot neglect. The reprint serves a most useful purpose for it renders accessible a large amount of work that would otherwise not be easy to get hold of.

J. O. Wisdom

The Principles of Mechanics Presented in a New Form. By Heinrich Hertz. Preface by Hermann von Helmholtz ; authorized English translation by D. E. Jones and J. T. Walley ; with a new introduction by Robert S. Cohen.
Dover Publications, New York, 1956. Pp. xl + 271. $3.50 cloth, $1.75 paper.

This new edition of the English translation of Hertz's famous book on the foundations of dynamics is doubly welcome, for not only does it make this classic generally available again and at a very reasonable price but it also contains a new introductory essay by Dr Robert S. Cohen.

Heinrich Hertz, like the late Enrico Fermi and the incomparable Isaac Newton, was remarkable for being equally gifted both as an experimental and as a theoretical investigator. Furthermore, as Professor Braithwaite has recently maintained, of the nineteenth-century physicists he was the most philosophically profound. In the final years of his tragically short life he centred his attention on the critical reformulation of dynamics, i.e. he endeavoured to do for classical mechanics what Hilbert set out to do a decade later for pure geometry. Dissatisfied with the vagueness and ambiguity of Newton's concept of force (denoting both inertial properties of, and external influences on, a body) and also with the broadly similar difficulties associated with the concept of energy, Hertz revealed himself as a philosopher of science in the best sense. For he not only submitted to logical analysis these two methods of formulating classical dynamics but in turn was stimulated to construct an alternative version (based on the three primitive concepts of space, time, and mass and the postulation of unobserved ' atomic ' masses, his definitions of force and energy being purely nominal).

As Dr Cohen points out in his admirable introductory essay, Hertz's own *Introduction* to his book is a classical work in the philosophy of science, for in thinking out afresh for himself a satisfactory foundation for dynamics he formulated penetrating criteria for the axiomatic reconstruction of empirical science in general. Cohen contrasts Hertz with Mach, pointing out that for Hertz laws of nature are not primarily descriptive shorthand for experientially correlated perceptions but rather prescriptive interpretative symbolic systems.

Hertz's greatness as a philosopher of science is evident on almost every page of this Introduction. He anticipated much that has become part of the mental climate of our time, e.g. his concept of the 'systematic grammar' of mechanics, his exhortation to us to distinguish in our theories 'between what arises from necessity in thought, what from experience, and what from our arbitrary choice', his warning that 'that which is derived from experience can again be annulled by experience', and his clear recognition that 'we have accumulated around the terms "force" and "electricity" more relations than can be completely reconciled among themselves . . . by removing the contradictions, the question will not have been answered but our minds, no longer vexed, will cease to ask illegitimate questions'.

G. J. Whitrow

Meetings of the Philosophy of Science Group

The following meetings were held during the academic year, 1956-57. They took place, by kind permission, in the Joint Staff Common-Room, University College, London.

1956	
8 October :	Chairman's address (Dr G. J. Whitrow), 'Some Reflections on the Philosophy of Science of Sir Edmund Whittaker'
12 November :	Dr W. R. Ashby, 'Basic Strategies of Research in Biological Systems'
10 December :	Mr N. R. Hanson, 'Newton and the Logic of Discovery'
1957	
14 January :	Dr J. Bronowski, 'Choice of Inductions'
11 February :	Professor C. D. Darlington, F.R.S., 'Determinism and Uncertainty in Genetics'
4 March :	Annual General Meeting. Professor S. Körner, 'Reference, Vagueness and Necessity'
29 April :	Dr. M. H. Pirenne, 'Optics and Painting : Leonardo's Problem'
20 May :	Dr J. R. Smythies, 'The Notion of Image-Space'
17 June :	Professor M. Kendall, 'On Probability'

Omission

The Note and Comment by Professor K. R. Popper, on 'Irreversibility : or, Entropy since 1905' published in the previous number, should have, of course, contained a reference to its having been read at the first Annual Conference of the Philosophy of Science Group, Manchester, 23 September, 1956. The Editor regrets this omission.

ABSTRACTS

Dialectica, 1956, **10**, No. 3

F. Moch, ' La logique des attitudes '

A two-value Logic terms a phrase a ' proposition ' only when it may be taken as an (abstracted) object for speech : its characters must be compatible. Accepting this rule, one may extend to the utmost the notion of a set, and formulate the relation between Logic and Set Theory, as well as when using the ' Logic of Composition ' (*Dialectica* 35/36). A three-valued ' Logic of inconceivable ' is implicitly used, which concurs with Mme Février's Logic, but cannot be reduced to the Logic of Composition.

A ' Logic of Attitudes ', which embraces all previous logics, may be defined ; the way to this Logic is a journey through Idoneism.

M. Polanyi, ' Pure and Applied Science and their Appropriate Forms of Organization '

There are two fundamentally different ways by which the full-time activities of a number of individuals can be co-ordinated ; namely (1) by subordination to a common corporate authority, (2) by mutual adjustment. Original contributions to pure science can be co-ordinated only by the latter method : each mature scientist must be given the opportunity to adjust his projects to the results freely communicated to him by all the others. Original work in technology will normally be carried out less freely, for it forms part of an industrial process for which the manager of the business carries ultimate responsibility.

J. W. Conway, ' Some critical Notes on the fundamental Relationship between Philosophy and Psychology with special reference to certain metaphysical implications '

Une résumé de cet article a paru dans les Actes du Congrès International de Philosophie des Sciences de Zurich, 1954 (Editions du Griffon, Neuchâtel).

Philosophy of Science, 1957, **24**, No. 1

H. Törnebohm, ' Epistemological Reflections over the Special Theory of Relativity and Milne's Conception of Two Times '

The metrical principle in the special theory of relativity is only partly supported by the evidence for the light principle. The nature of the support required is discussed.

Part of the construction of a time-metric consists in the specification of a time-scale. The problem of time-scales which has been thoroughly treated by E. H. Milne is discussed in a second part of the paper.

The paper contains two points of special philosophical significance : firstly, how interpretations of formulae shift when we move from laboratory systems to galactic ones, and secondly, a distinction made between an operational definition of a physical variable in terms of measurable variables and a physical interpretation of a variable in terms of concepts defined within a theory.

M. Capek, ' Note on Whitehead's Definitions of Co-Presence '

There is a certain discrepancy between two of Whitehead's definitions of co-presence. In *The Concept of Nature* the relation of co-presence is defined as a relation between two events situated in the same instantaneous space in one particular frame of reference ; in *The Principle of Relativity* the same term ' co-presence ' designates the whole four-dimensional area containing all events causally independent from the ' Here-Now ' of one particular observer. But the illustration used in *The Concept of Nature* clearly indicates that it was the second meaning which Whitehead already then had in mind. Two successive events, which are causally connected, cannot become co-present in the sense of the first definition for any observer while they may become so in the sense of the second definition. In his illustration Whitehead claims that two successive dates of the earthly history, for instance, the birth of Queen Victoria and the date of writing his book in 1920, can become co-present for some very distant observer. But as these two events are causally related in the relativistic sense, they can *never* lie in the same instantaneous space for any observer. On the other hand, for a sufficiently distant observer they are included in the interval of causal independence which, according to the second definition, constitutes ' the region of co-presence '.

D. L. Miller, ' The Importance of Presents in Contemporary Science '

Contrary to the views (1) that the present and the future must be understood through an analysis of the past and (2) that the past and present must be understood by way of a future or through final causes, this article defends the thesis that both past and future must be understood from the standpoint of a present.

H. Wein, ' Trends in Philosophical Anthropology and Cultural Anthropology in Postwar Germany '

' Anthropology ' in Germany has usually been understood as biology and genetics of Man. There is no field of research exactly corresponding to what is called ' cultural anthropology ' in the U.S.A. and England. On the other hand, the philosophers and sociologists at Cologne in the twenties, Max Scheler and Helmuth Plessner,—also trained in biology—coined the term ' Philosophical Anthropology ', meaning a synthesis of all the modern sciences dealing with Man. Special questions raised in some German publications are the following : can we determine the limits of the initiative and planning power of the individual within society and historical process ? Can and should the value-orientations of societies and cultures be changed by political or scientific methods ?

H. Finch, ' Validity Rules for Proportionally Quantified Syllogisms '

The classical rules for the validity of syllogistic inference are applicable only when premises and conclusions are propositions whose structure involves the quantifiers ' all ' or ' some '. In universes which are composed of measured sets, it would be useful to determine the validity of syllogistic inference utilising rational numbers in quantifying phrases, for example, to determine whether the syllogism ' at most 1/3 m are p, and at least 1/2 m are s, therefore precisely 2/3 m are p ' is valid. On the basis of the axiomatics of measured sets, six rules of validity for proportionally quantified syllogisms are derived which generalise and formalise the norms of numerically definite syllogistic reasoning stated by Boole, De Morgan, Yule, and others.

Philosophy of Science, 1957, **24**, No. 2

E. Gellner, ' Ideal Language and Kinship Structure '

This article tackles the philosophical problem of an Ideal Language, i.e. a notation which describes the world without redundancy of terms, ambiguity, or misleading projection ; and at the same time the crucial anthropological notion of Kinship Structure. It tries to specify the general conditions an ideal notation would have to satisfy, to convey the type of information contained in anthropologists' descriptions of kinship systems.

K.W. Spence, ' The Empirical Basis and Theoretical Structure of Psychology '

The present paper is concerned with two aspects of the language hierarchy of psychology : (*a*) the abstract terms and statements at the higher end of the hierarchy that provide for theoretical integration of behaviour data and (*b*) the set of descriptive or observational terms at the lower end of the hierarchy that provide the empirical basis for introducing the theoretical terms. The methodological requirement proposed is that of inter-subjective agreement. Objective data or public experiences, such as are designated by Carnap's physicalistic terms, meet this criterion, whereas the subjective data or private experiences obtained by introspection do not.

Mentalistic terms referring to more complex aspects of personality are quite permissible in psychology so long as they are not among the elementary, undefined terms of the basic observational vocabulary, but rather are defined or introduced in terms of a class of basic terms that meet the methodological requirement of high intersubjective consistency.

The present status of psychological theory was contrasted with theory in physics. The nature of one type of theorising, the use of intervening variables in learning theories, was discussed. The rôle of this type of concept is primarily to integrate and bring into deductive relation with one another the many different, highly specific, laws found in the various kinds of learning situations.

L. J. Goldstein, ' The Logic of Explanation in Malinowskian Anthropology '

The method of explanation characteristic of almost all of Malinowski's attempts to deal theoretically with cultural phenomena is analysed. It is Malinowski's contention that the form of any cultural phenomenon is determined by its function. Careful analysis reveals that Malinowski has begged the question. His definition of the notion of form includes that of function, making his desired result follow trivially. It is further shown that certain well known Malinowskian doctrines, in particular his opposition to historical methods in anthropology, are logically dependent upon his form-function thesis. The next part attempts to show how Malinowski applied his method to actual instances. However interesting and suggestive are Malinowski's views about magic, language, and other cultural phenomena, the circularity of his basic formulation makes it clear that he cannot be said to have established his point of view.

E. H. Madden, ' Aristotle's Treatment of Probability and Signs '

Aristotle often used the concept of probability but made no persistent effort to clarify or analyse it. In *The Prior Analytics*, where he discusses the enthymeme (70a, 5 sqq.), he develops a somewhat statistical interpretation of probability.

Aristotle sometimes refers to *endoxa*, or currently accepted opinions, as probabilities ; but while *endoxa* may *be* probable, it is clear that Aristotle does not intend ' currently accepted opinion ' as an analysis of ' probability '.

There appears to be no trace in Aristotle. His theory of induction and his notions of ' sign ' and ' cause ' prevented him from interpreting probability as degree of confirmation of hypotheses.

A. Pap, ' Belief and Propositions '

The sense in which cognitively synonymous sentences are said to express the same proposition, may be given by an axiomatic definition of ' cognitive belief '. This would amount to a formalisation of the traditional conception of propositions as possible objects of belief. The possibility of explicating ' synonymy ' is defended against recent scepticism. Discussing Quine's problem of ' ontological commitments ', a semantic interpretation of such ontological statements as ' there are propositions ' is suggested.

W. C. Salmon, ' The Predictive Inference '

By a ' predictive inference ' is meant an inference from a relative frequency in a finite observed sample to a relative frequency in a finite unobserved sample of a probability sequence. One way to treat this kind of inference is to regard it as a double inference, first, from the observed sample to the long run probability, and second, from the long run probability to the unobserved sample or short run. Regarded thus, the predictive inference involves two kinds of rules, an inductive rule for estimating the long run probability from the observed sample, and a short run rule for estimating the relative frequency in the short run from the long run probability. Reichenbach attempted to give a ' pragmatic ' justification for an inductive rule and Salmon (' The Short Run ', *Philosophy of Science*, 1955) attempted to give a similar kind of pragmatic justification for a short run rule. If the probability sequences in question are infinite, the suggested method of treating the predictive inference leaves it completely arbitrary on two counts.

J. Kaminsky, ' Can Essence be a Scientific Term ? '

Some contemporary philosophers have argued that essence is a permissible scientific term. This position is invalid for three reasons : (*a*) Any propositional formulation of essence designates a state of affairs which is in principle incapable of empirical verification ; (*b*) The traditional statement of essence entails a necessity that would not be allowable in a scientific context ; (*c*) A possible scientific use of essence is based on the view that scientific laws are approximating an ideal non-corrigible natural form.

RECENT PUBLICATIONS ON THE PHILOSOPHY OF SCIENCE

(*a*) Books Received for Review

O. Barfield, *Saving the Appearances*, Faber & Faber, London, 1957, pp. 190, 21s.

G. A. Beltran, *El Proceso de Aculturación*, Universidad Nacional Autonoma de Mexico, 1957, pp. 269

G. Bergmann, *Philosophy of Science*, University of Wisconsin Press, 1957, pp. xiv + 181, $4.75

John W. Blyth, *A Modern Introduction to Logic*, Houghton Mifflin Company, Boston, 1957, pp. xvi + 426, $ 5.50

M. Born, *Atomic Physics* (sixth edition), Blackie & Son, Glasgow, 1957, pp. xiv + 445, 40s.

L. de Broglie, *Mécanique ondulatoire du photon et théorie quantique des champs* (2nd edn. revised and corrected), Gauthier-Villars, Paris, 1957, pp. vi + 208, 2,900 fr.

H. V. Brøndsted, *The Atomic Age and Our Biological Future*, Watts, London, 1957, pp. xiv + 80, 9s. 6d.

P. F. Carcano, *La metodologia nel rinnovarsi del pensiero contemporaneo*, Libreria Scientifica Editrice, Naples, pp. xi + 450, L. 3,500

Ernst Cassirer, *Determinism and Indeterminism in Modern Physics*, Yale University Press, 1956, pp. xxvi + 227, 40s.

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